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EDITORIAL.

RELIGION AND MEDICINE.

A book under the title, "Religion and Medicine,"* written by Worcester and McComb, two clergymen, and Coriat, a physician, has excited much comment, as also has the experiment upon which the book is based and upon which some of its conclusions are founded. It is difficult to determine just how to view the book,—whether as a piece of literature, as a medical treatise, or as a philosophical exposition of the problems clustering about the subjects of soul, belief, prayer, faith, religion and sin. It is not a report of the results of the treatment which the Emmanuel Church in Boston originated for the cure or betterment of nervous disorders. It certainly has no clinical appeal to medical thinkers nor does it present anything particularly striking in the psychological field. It is apparently designed to teach the public, that is, the more intelligent public, the appreciation of the fact that nervous disorders are the result of various perverted ways of living and thinking, and that they can be cured or improved by modifying the surroundings and by strengthening the moral attitude by an appeal which, in a general way, is invested in the activity of the church. It means that this activity is as much a specialized part of the church's function as is the medical advice that of the physician's, the aid of the latter in this connection forming the one unique feature of the whole experiment.

Naturally a physician turns first to the part of the book which is written by a physician. Coriat is known as a writer of prominence in the field of morbid psychology and psychotherapeutics. He has attempted to present, for popular reading, chapters on the anatomy and physiology of the nervous system, diseases of the subconscious, the nature of hypnotism, etc. It can be said of these that they are no better nor worse than such chapters usually are. The disputed points in the complex

*Religion and Medicine. Moffat, Yard & Co.

chapter of knowledge are glossed over so that an intelligent layman after reading them would think that he knew more about subjects, such as the subconscious, the problem of consciousness, the nervous system, the nature of heredity, the function of a nerve cell, the nature of hypnotism, than a well informed specialist in these departments. Coriat seldom admits that there is a doubt on many of these questions. Issue might very well be taken with Coriat's statement that hypnotism is a well recognized therapeutic procedure. There is too much difference of opinion on this subject to warrant such a conclusion as this, and it certainly seems out of place in a popular treatise. It remains to be seen if this method of treating the diseases mentioned is not followed by serious results. Coriat plainly shows here his leaning toward the position which is especially associated in this country with the work of Prince. He seems likewise to give little or no attention to some of the recent work on the subject of neurasthenia, and he has certainly no warrant at the present for giving to it the prominent place that he does. It is plain, therefore, that, as far as the medical aspect of this is concerned, it suffers from the same kind of fault that is common to popular expositions on medical subjects the world over. The chapters written by Worcester on the subconscious mind, suggestion, auto-suggestion and the cause of nervousness, form the best part of the book. There is a certain logical unity to them that makes a refreshing contrast to the rest. It is an intelligent treatment of a very complex subject, adapted to the capacity of the average well informed layman. The latter chapters of the book, which deal with such subjects as faith, prayer, the healing wonders of Christ, the outlook of the church, suicide and its prevention, etc., are written by Dr. McComb. They are in the main frankly religious; by that is meant they concern themselves less with questions of fact or the interpretation of carefully observed phenomena, than with questions concerning which a certain amount of previous belief is necessary. This previous belief is not a matter of definite quantity, so to say, which is characteristic of the scientific method and exists independently of the individual, but is the personal reaction of the individual to the questions which are touched upon. There is less place for criticism in such chapters. The subjects are handled in a rather restrained manner; a thing that merits praise as their character might excuse a much more intense method of treatment.

What shall be said of the book as a whole? The larger question which it raises is the place of religion in medicine, and the value of organized effort from the point of view of the church. The question must naturally arise,—Is the effort worth while? Are the results sufficiently good to warrant a further development of the idea? There is, further,

this thing to consider,—how much of such an attempt is an unconscious reaction against the mysticism and the false philosophy, and, let us then add, the popular success, of Christian Science? All these are vital questions and it is impossible perhaps to answer them all, or even any of them, definitely. It is difficult to adopt a satisfactory point of view. There are so many phases to the question. If the church in its feeling of hopelessness in respect to the religious appeal in the narrower sense, attempts to add to its forces a therapeutic branch in order to hold its recent recruits, the obviously utilitarian point of view is the correct one. It becomes almost a part of the new philosophy, called "pragmatism." Will it pay? Will it be useful? If so, it is true. There is in this book an almost conscious admission that there are many sincere religious people who hold this belief. Now medicine, as an art and a science, can have but little place in any such scheme of self-protection. For the moment medicine departs from the straight path of science, or rather what may be termed the scientific method, that moment it is in danger of losing its most valuable present day asset.

There is no question that religion and medicine have never mixed without both suffering; the history of medicine and its long struggle with mysticism is too well known to need further comment. The danger of this may be pointed out perhaps more clearly by considering the fact that functional diseases are functional just so long as an adequate cause and an adequate variation from the normal anatomical structure has not been discovered. Now if the therapeutic effort of the Emmanuel Church limits itself practically to the diseases which are called functional, it is clearly seen that its therapeutic activity is but a temporary shift, awaiting merely the discovery of the cause and the pathology of the disease. If, as soon as a disease becomes organic in nature, the effort ceases, then the therapy is absolutely illogical, because people suffering from organic disease are as much in need of the brightness and hope which the church brings to them as are the sufferers from purely psychical troubles. The contention is now made that a religious form of therapy must not be weakened by concerning itself with the sort of disease it attempts to treat. The decisive point is just this: People with organic diseases of the nervous system, particularly, and other sorts of chronic diseases, likewise need something decidedly more than the sort of mental therapeutics that the Emmanuel Church offers them. They need, apart from the medical side, a firm belief and a firm conviction in the very mystic power which the church, as it is now constituted in this particular instance, does not aim to supply. They need religion in the very primitive fashion; in its elemental unpsychological form, it may be said; for it is only upon some

such crude reliance that they obtain comfort. It is the purely intellectual attitude of the Emmanuel Church movement which is probably its weakest point. The very fact that the therapeutics which this movement proposes, is limited to the functional class creates an artificial distinction to which disease itself pays little or no attention. It is probably true that in cases taken by and large, the physician working on one side and the priest on the other, both coöperating as chance occurs, makes a better and more effective power. One can see and perhaps feel that in the Emmanuel Church movement there is a chance of weakening both the physician and the church. Whatever else may be said, the two look upon disease in an entirely different way, or it might be said with a different vision. It is becoming more and more evident that the biological conception of disease is the most logical. The moral conception is too evidently a growth of the medieval attitude. It seems conclusive that the church can only be a successful therapist if it adopts the old notion. No matter how it dresses itself in the nomenclature of pathology, beneath its outward scientific trappings it is still the spiritual and moral force, relying upon the power of tradition, the faith that knows no reason, and the fear of the final result. No amount of introspection or analysis, no amount of psychology, no number of formidable terms, will suppress the fact that when the human soul is tired and alone it needs the comfort which the primitive man, in common with his civilized brother, needs; he wants a feeling that there is somewhere a greater power than his which, after all, is chiefly concerned with his future and will reward or punish according to some scheme of justice beyond his comprehension. The religion which feeds his primitive need will be the final one for those that deem religion necessary. For such a religion, there is no necessity for an effort such as the Emmanuel Church is now making in the treatment of disease. Of greater interest than the question of the utility of this effort, or of its prominence, is the query which concerns itself with the interpretation of the whole movement. What it means is of greater significance than what it is and what its future will be. There is no doubt that movements of this nature can not be viewed as solitary phenomenon. While the Emmanuel Church effort is perhaps not a thing big enough to have a world significance, it represents, even in a restricted sense, a certain reaction to forces which are at work in the world at large. There is no doubt that we are in the midst of a large moral or ethical evolutionary phase. The extent and significance of this cannot be properly viewed by those in the midst of it. There is a swinging away from the materialistic point of view in the sense of some kind of yearning towards the better appreciation and understanding of the idealistic conception of things. The use of the

term "idealistic" must be explained. By this is meant not idealism in the sense of something superior and highly moral, but in the sense of its original conception that thing which is concerned chiefly with the world of ideas. The world is full of these currents. The peace movements, the efforts at civic reforms, the insistence of the higher virtues in government affairs, the prohibition movement all over the world, and, above all, the establishment of new and strange religious sects, all of which, even the crudest,—Christian Science,—mumble the half forgotten concepts of the eighteenth century idealistic philosophy. Psychotherapeutics, that remarkable awakening in medicine of the past few years, means that this stimulus has reached the sluggish current of medical thought, the last to react to world movements of any kind.

If we view, then, the Emmanuel Church therapeutics from this point of view, we will see in it a very vital thing. It is merely a part of a more universal tendency, and its importance to medicine is really of small consequence. It solves no problem of disease and it forms no new conception of therapeutics. The physicians who are a part of it are deluding themselves if they think that either medicine or religion will in the long run be influenced by it. It helps some of them to get a closer view of the human soul, but surely not closer than can be obtained by methods in use by neurologists all over the world. To many the church is an obstacle to free confession; to some it is a help. To the latter, the Emmanuel Church will appeal, or some similar institution. To the former, there will still continue to be the same grim struggle against disease, the same effort to grip the life that is inherent in them and to surrender with regret when the time is due. What Emmanuel Church is doing many a physician and nurse, and many a one who is neither, is doing the world over.

The phenomenon of life is largely an exposition of the struggle against death, and disease is but the definite mechanism by which death is always victorious. All efforts to increase the resistance of the living, to increase the power to combat disease and to add to the delight in normal thinking and acting, should be upheld. If the Emmanuel Church does this it is a good thing. An admission of this does not emphasize its importance. That is the main contention of these paragraphs.

ANNUAL ORATIONS AND ADDRESSES.

The pride that comes to one who has the honor of being selected from the mass of his professional brethren to deliver an annual oration or an annual address, must of necessity be tinged with a sense of self-sacrifice and impending gloom. For we who listen to the chosen orators of our profession compose a critical audience which is all too apt to treat in a superficial way the focused thoughts of a year's hard work, and too ready to condemn or praise the orator from the impression which he has produced upon us in an intensely crowded and hot auditorium. Several facts enhance this critical attitude: That association meetings are held in summer weather; that so much is being read and spoken that one gets saturated by simply listening; and that an annual address or oration is, as a rule, a very serious consideration, in a lengthy manner, of some very important subject. To the average listener the annual address as delivered carries but little weight. The average reader, however, gains much by a careful perusal of this painstaking and prepared essay. An editorial abstract, indeed, contains much food for thought. This seems particularly to be the case this year, some addresses having been delivered which are worthy of several readings and much careful consideration. These essays might be spoken of, to borrow a political phrase, as *key-notes* of our present position.

First among these essays is that of Dr. Charles Harrington, on "State's Rights and National Health;" an essay developed in clear and forcible English and carrying its message to Garcia. One can not be too seriously impressed with the thought developed by Dr. Harrington, that we are an unscrupulously commercial people; a people unaccountably blind to the pecuniary loss suffered annually through sickness and death due to preventable disease; but, nevertheless, a people acutely awake when disease threatens our animals or plants. He points out clearly that a yellow fever outbreak, such as that of 1905 in New Orleans, does not cause the national government to loosen its purse strings, while an outbreak of animal disease, such as epizootic, or foot, or mouth disease, will be met with the expenditure of perhaps a million dollars. Dr. Harrington, however, is not simply fault-finding, but recommends to the consideration of each medical man throughout the United States, the establishment of a medical board of health; a body composed of men of recognized standing in medicine, engineering or other branches of science that will be of assistance, called together once a year by the head of the bureau to consult and advise about health matters; in fact, composing a council of delegates, one from each state appointed for a term of years by the Governor or State Board of Health

Well may Dr. Harrington ask, does the profession in general perform its whole duty to the community when it fails to show any concern about who shall be the representatives of preventive medicine? Well may he change the substantive of Matthew, x, 31, "Fear ye not therefore, ye are of more value than many swine."

The national government is proverbially stingy in the question of salary, and to its liberality on the question of rewards for splendid service, let the ghosts of Reed, Carroll and Lazear rise and tell. All this must be changed in time, and it seems that the medical profession is asleep on its own rights.

The annual address of the President of the American Medical Association, Dr. Herbert L. Burrell, strikes the key note of public education as a means toward aiding scientific medicine. He points out that the public have amassed much knowledge of disease which is indeed in a chaotic condition due to the indiscriminate way that articles upon medicine are dosed out by the lay press to a demanding and vitally interested audience. The idea enthroned in this address is that the tendency of the age makes it necessary for the medical profession to give out at first hand from authoritative sources, such facts of medical science as have become established so that the laity may be in a position to understand the advantage of the general movements toward sanitary legislation and preventive medicine.

Dr. William Sidney Thayer has also struck this note in his oration on medicine. He says: "Here we are, then, a great body of medical men, united in an association, not for our personal, financial, political or social advancement, but solely in the cause of truth and for the benefit of humanity,—the deliverance of the public from disease. Nevertheless, the work before us is unending, the obstacles great. But why should there be obstacles? Why have not the recommendations of this association the force of law? Why is it that, when laws themselves exist, all our strength is needed to secure their enforcement?"

The main cause, I take it, of all this is the lack of comprehension on the part of the public of the nature and aims of medicine, and of physicians. If the public had but a small part of the information which is ours, how many thousands of lives might be saved every year! The community as a whole has a very vague understanding of the nature of the medical sciences and arts. Under these circumstances can we expect such a public to sympathize with and blindly follow all our recommendations? Surely not."

Dr. Thayer also points out that the physician has no higher public duty than the duty of simplicity, the duty of avoidance of mystery in medicine and the duty of truthfulness; these three bearing much influence on what the physician shall be able to teach his non-medical friends.

At no time in the history of the American Medical Association have addresses of this nature been so pregnant of meaning, and it is imperative that they should not be cast aside with merely a glance, as is too often the case with lengthy addresses; and that each of us should endeavor to follow the thought contained, and apply the knowledge so compactly given to our daily efforts in seeking the general advancement of our work as medical men.

THE MEDICAL VIEW OF ANARCHY AND SOCIALISM.

The psychology of anarchy must receive more publicity. Every little while there is an effort to enact laws to eradicate it, but they are more like purposeless spasms, for they have been barren of results. Failure seems to be due to the fact that legislators do not understand what it is they are dealing with, though to the psychologist the matter should be as clear as daylight. The doctrines of the anarchist are so amazingly illogical that it is quite evident his brain is far from the normal. It is not necessarily a small or feeble brain and the condition is therefore not allied to idiocy, imbecility or feeble-mindedness. Nor indeed is insanity present for there is positively no evidence of disease such as is found in the case of paranoia, but it is allied to that disease as it is a degenerative condition. Like paranoiacs, anarchists are often very active and keen mentally but they differ in this, that whereas the paranoiac may reason logically on his false ideas or delusions as premises, the anarchist reasons illogically upon well established facts. Insane brains may be badly deranged in function but such degenerate ones are badly arranged in material.

"Scientific Anarchy" has been quite extensively written up by Kropotkin who has enriched science in many ways but it is a matter of extreme regret that he should use the word anarchy now that degenerate assassins have appropriated it. The word was adopted by democratic revolutionists who were oppressed by an unduly severe government. They fought for the rights of men as opposed to the rights of the group of men called the social organism. The two sets of rights are somewhat antagonistic for though society has evolved to preserve the greatest number it sometimes calls for the death of many of its members. In war many must die that the rest may live and the rights of the individual to life itself practically disappear, but in peace those rights are enhanced to the highest degree, as the preservation of life is the reason for the existence of society. Kropotkin describes the rights of man but makes the amazingly unscientific mistake of considering those rights so su-

premise that any check upon them is tyranny of the mass, whereas it is an axiom that personal liberty is limited when it injures some other person's liberty or hampers the efforts of the mass to preserve the greatest number. In his view and that of the original anarchists the mass had no rights as a mass and "government" or the agent of the mass for their preservation was pure tyranny. Of course it is all absurd though the most natural thing in the world to arise where "government" had gotten into the hands of a few who used it more for their own advantage than for that of the mass. The same oppression existed in American colonial days, but anarchy was not preached. Our revolution was a struggle for a government which would bear upon all alike, not for abolition of government without which society melts and the units kill themselves off as in prehistoric days. We must be protected against ourselves.

Assassination of governmental agents was assumed by the illogical anarchists to be the best means of ending oppression, wholly ignoring the facts that these are merely agents of the mass and that without their protection even the anarchists themselves would die. After the very act of killing the police, they demand the protection of other police. After denouncing courts they throw themselves upon the courts for protection. Their literature is so full of other vagaries, inconsistencies and illogical reasonings that there is not the slightest doubt of mental abnormality of sufficient degree to warrant confinement in asylums. Whether those who preach murder should not be executed is of course open to debate, but that they should be removed from society in some way is self evident. It is a matter in which psychologists might do a world of good by more frequent discussions of degenerate or warped minds,—to the end that legislators may find a way of removing anarchists from the possibility of doing further harm. McKinley was killed by one of these perfectly sane but warped degenerates, and of the hundreds of propositions to curb them, not one has been practical and nothing has been accomplished. The medical profession must lead the way and the need is urgent.

Socialism need not bother us very much, for though it is the very opposite of anarchy and just as unscientific, it is being used as an instrument for lessening disease and its workers can be encouraged in their efforts to benefit the least efficient. Socialism really advocates an enormous extension of the powers of government wherein all are to be treated as though there was perfect equality. Society is assuming family duties so greatly that we are becoming more and more socialistic every decade—public schools in place of home instruction, kindergartens instead of home nursing, trade schools instead of private apprenticeships, and boards of health to enforce all manner of things, but the ideal of abso-

lute equality and the prevention of exceptional rewards to exceptional men is quixotic as it ignores the fact that society exists simply because each unit struggles for what his ability permits. Nevertheless, the medical profession is brought into such close relations with the practical sanitary and benevolent works of socialists that we might admit some good in it though it does immense harm by stirring up discontent with one's congenital defects, and for this reason it has unfortunately been made the host of anarchistic parasites, and it would be best to drop the agitation. In Germany it is merely a cry of the undemocratic masses for greater paternalism of government, and we have it here under other names in every political campaign, the word socialism being unnecessary. The individualism of England will not tolerate it, though it is growing to an extent which must be reckoned with. There is a possibility that medical attendance when sick will be declared a duty of society and physicians might become public officials. The dispensary evil has gone a great way in that socialistic direction, and in England there are many observers who are quite convinced that in time, there will be as few private physicians as there are private pedagogs. Nevertheless socialism though coming quite close to all classes as well as physicians, is so unscientific in its ultimate ideals, and so misused by the degenerate, that it might as well be dropped for good and all.

LODGE PRACTICE.

Contract practice seems destined to loom up as large in American industrial centers as it has in English cities. It appears from a paper by Dr. Julius Weiss (*Med. Rec.*, April 11, 1908) that almost 75 per cent. of the East Siders of New York City are insured for medical attendance in lodges or societies. The result is an unbecoming competition on the part of young physicians who have not yet established a practice or who have failed. The evils are self evident, but it is not at all unlikely that they will correct themselves in time. The British Medical Association investigated the matter quite extensively some years ago, and the general impression subsequently gained is to the effect that the evils are not as great as at first supposed. One thing was quite clearly brought out and that was the fact that the sick got just about what they paid for and no more. A person intelligent enough to provide for the future is quite apt to wish to select his own physician when sick, and not depend upon the lodge doctor. In time they seem to feel that it was better to employ those whom they wanted, and as far as we know they show a tendency to withdraw from the associations. Of course there is a gambling element in it, for they feel that they are apt to get some-

thing of great value for a small outlay, and it is this possibility of insurance which attracts them in such large numbers at first. No doubt it would be better to put the money in a savings bank than in a policy shop, but that is expecting too much of human nature. The tremendous advantage of the system is the fact that the poor feel that they are getting what they paid for, and there is a total absence of the pauperizing tendency of the old style free dispensary—still too numerous with us. If all people felt that it is society's duty to care for them when sick, then physicians will become public officials, like teachers, but that state is too far off to be more than a subject for academic discussion. At present, free medical care has a degrading, pauperizing effect, and the victims will not look out for themselves. Lodge practice is a distinct advance and as it is apparently with us to stay, it must be reckoned with. It will probably not become any more extensive than in England, for the lodge members, as they become better off, will prefer to select their own family doctors and pay for the advice as they pay for anything else they want. All such matters are governed by the natural laws of supply and demand and it is not quite certain that unionism methods urged upon physicians will alter the conditions in the slightest degree.

LITERARY NOTES.

Pierre Pic is a Frenchman who has ingratiated himself with his countrymen on more than one occasion by editing rare editions of works bearing on medicine, in which is illustrated the true Gallic spirit. In his new book, "*Pilules apératives à l'extrait de Montaigne, préparées ad usum medici*" (Aperative Pills made of the Extract of Montaigne, prepared for the Use of Doctors) he not only evinces a deep knowledge of the incomparable essayist's works but a veneration which can easily be construed into the sort of affection which makes even a temporary separation from a favorite author's works a real disaster. That Pierre Pic has found in Montaigne's works many passages of more than ordinary interest for the medical world is explained in a witty fashion in the preface, from which we quote the following: "These pills are not in any way the same as the pills in the French codex nor are they applicable to all temperaments; their size varies; their dosage can not be regulated. Certain ones are such large boluses that it must needs be a strong stomach to make deglutition possible where bitterness is so thoroughly undisguised. Assuredly, their taste savors more of aloes than of powdered licorice."

Montaigne is without doubt an old friend of many members of the medical profession—the sort of companion one gives up reluctantly, so varied are his talents. This is not surprising, for of all the older writers he is the most modern; in fact, his mentality is even to-day, akin to our own. Again what author in the whole range of French literature displays a similar ingenuity and originality of thought—qualities which hold us constantly in thrall. Knowing Montaigne fairly well it is with con-

siderable delight that we open this new book filled with excerpts, for not only are the selections of the best, but their arrangement is admirable; and moreover, the convenience of finding the aphorisms which we have half-forgotten without the trouble of resorting to the original works, can not be overestimated. When we recollect how pithy, acrimonious and mordant Montaigne's sentences are, we feel no hesitancy in recommending them to all doctors who, on account of many medical experiences, have envisaged life in all its many phases.

That baffling, picturesque and romantic figure in literature, Lafcadio Hearn, is the subject of a sympathetic interpretation in Dr. Geo. M. Gould's new book "Concerning Lafcadio Hearn" (Philadelphia: George W. Jacobs & Co.). Seldom, indeed, has a writer of Hearn's well-known peculiarities received kinder treatment, for not only does Dr. Gould bring to bear on the matter a mind instinct with latitude and justness, but his critical sense stands him in good stead in dwarfing as far as possible the glaring defects in the man's make-up, so that the high literary values of his work stand out with prominence. Whether it was Hearn's unhappy youth, his direful experiences in Cincinnati—direful because of his poverty and an environment uncongenial to his exuberant and poetic imagination, his extreme myopia (this according to Dr. Gould), or the confounding of the Irish and Greek strains in his being that must be held accountable for his vagaries, we are not in a position to say; but the fact remains that as a personality he was as unattractive as he was fascinating as a writer. Ingratitude writ large dominated all those qualities which make for the amenities of life, and that Dr. Gould can throw the mantle of charity over so great a blemish, though he himself was the recipient of treatment quite despicable, speaks well for his forbearance. Fortunately, the book is not all about Hearn himself, but—and here Dr. Gould's critical sense comes in—an excellent interpretation of Hearn's unusual mind and the spirit which informs his work. The chapter "Getting a Soul," shows us distinctly that though Hearn did good work in New Orleans and later in the French West Indies, it was not until he went to Japan—and this by the way he did at Dr. Gould's suggestion—that he really found himself: that is tore himself away from certain French writers whose influence had been an obsession, and wrote according to the dictates of his unbiased original mind. Students who are interested in all the quiet but, at the same time, potent forces which underlie the social fabric of a nation, should not neglect Hearn's books on Japan, for they are both instructive and illuminating. Dr. Gould's book, while candid to a degree that might shock our literary Philistines, is a manly performance and superior in every sense to Elizabeth Bisland's "Life and Letters," in so far as we are not only brought face to face with Hearn himself, with his high lights and low dim ones, but get an insight into the workings of his extraordinary mind and an excellent interpretation of his works.

ORIGINAL ARTICLES.

FOREIGN BODIES IN THE ABDOMINAL CAVITY.*

By H. S. CROSSEN, M. D., of St. Louis.

Whenever the dangers of abdominal section are mentioned, the mind naturally reverts to hemorrhage, to sepsis, to shock or to the failure of one of the vital organs under the post-operative strain. In all these directions great strides have been made in recent years toward effective preventive measures.

I call attention now, however, to an accident fully as serious as those just mentioned and yet one in which the preventive technique usually employed remains as ineffective as it was ten years ago. I refer to the leaving of sponges and forceps in the abdominal cavity.

To one not familiar with the subject it seems past belief that we would carry into the abdominal cavity any object, the removal of which is not provided for with absolute certainty. And yet, that such is the case is abundantly proven by the long list of cases in which sponges, forceps and sundry other articles have been removed from the abdomen, during convalescence or at the post-mortem table. Many cases have been reported, and there are many more cases that have not been reported, since, for obvious reasons, the accident is not given publicity unless there is some special reason for doing so. In any large body of surgeons, a little experience meeting in which testimonials are freely given, will show a number of cases in which this serious accident has occurred.

Of the reported cases in which a foreign body was left in the abdomen, the largest series that has come to my notice is that collected by Schachner. In an excellent article,¹ he reviews the 109 cases collected by Neugebauer and adds 46, making a total of 155. In about 70 per cent. of the cases, the foreign body left was a sponge or sponges, in 25 per cent. artery forceps, and in the remaining 5 per cent. drainage tubes, and finger rings and miscellaneous articles. Death resulted in approximately 40 per cent. of the cases.

SPONGES.

A gauze sponge or pad is the article most frequently left. Concerning this there are three questions which I shall consider briefly; (1) how does the accident occur; (2) what are its consequences; and (3) how may the accident be made impossible?

*Read before the St. Louis Medical Society, February 15, 1908.

¹Annals of Surgery, 1901, Vol. 34, pp. 499 and 678.

How does the accident occur? It is surprising how easily and quickly the intestinal coils will enfold an object and carry it out of sight and touch.

Perhaps the most common occasion of the leaving of a sponge in the abdomen is some condition that necessitates hurry in the closing steps of the operation. The patient is weak and failing and, consequently, very rapid completion of the operation is imperative. Some persons may think that even in such cases the accident is due to inexcusable carelessness. Such a view of the matter may seem very plausible to the inexperienced, but its erroneousness is painfully evident to those who have had personal experience in desperate abdominal cases, where the patient is wavering between life and death and a few moments delay may turn the balance. As the wound is being hurriedly closed, that the patient may be gotten into the warm bed and various restorative measures used, it is found in the counting that a sponge is missing. Where is it? Is it in the abdomen or is it in some of the various rolls of things about the table or in the waste container or on the floor. To search the peritoneal cavity again or to delay the closing of the abdomen until everything around the table is thoroughly searched, may cause the death of the patient, while to close the abdomen with the possibility that a pad or sponge is in there, is not less dangerous. This makes a most uncomfortable situation and one all too frequent.

Another occasion of this accident is in operating with unfamiliar assistants or nurses. Not knowing certainly the operator's custom, each one supposes some one else is keeping responsibly watch of the sponges. Consequently the exact number put into the abdomen and taken out, is not closely watched, and the general counting may be neglected until the abdomen is closed. Again, the sponges may be dropped about so carelessly, by unfamiliar assistants, that an accurate count can not be made promptly as the wound is being closed.

A third cause of this accident is a mistake in the number of sponges used. This may be made by the nurse preparing the sponges (an extra one or two being included in the set), or it may be due to the addition of one or two during the course of the operation, because of some pressing need. For example, in a recorded case, the operator called for another sponge. All the regular sponges having been used, the nurse hastily secured an extra one from the sterilizer and gave it to the operator. In the hurry, the fact that an extra sponge was in use was overlooked. At the close of the operation, the number of sponges tallied with the regular set. Several hours later the operator was informed that a sponge was missing. He replied that that was impossible as he used only a certain number and the set was complete at the close of the operation. He was then told that an extra sponge had been used and that it could nowhere be found. The abdomen was then reopened and the sponge was found up under the liver.

A fourth cause of the accident is a mistake in the final counting. The

prompt getting together and accurate counting of the two or three dozen sponges and pads, as the abdomen is being rapidly closed, is not such an easy task as might at first appear. A sponge is not infrequently reported missing and then, after a long search, is found somewhere about the table. On the other hand, the number may be pronounced complete and yet later, one be found missing. It is useless to say that this should not occur. The fact remains that it does occur and, under the varied circumstances of operative work, it will continue to occur as long as dependence is placed on the counting.

An esteemed confrere, whom we all know to be one of the most careful of men, has reported cases forcibly illustrating each of the two points just mentioned. In one case a sponge was reported missing and twenty minutes were spent in hunting for it before closing the abdomen, when it was finally found on the table after all. In another case, the number of sponges was reported "correct," and the abdomen was closed. An hour later an assistant, a conscientious and painstaking gentleman, said to him, "Doctor, I have counted the sponges again since the patient was put to bed and I miss a sponge. I think I counted them correctly before the operation." Three sutures were removed from the recent wound so that two fingers could be introduced into the cavity and, after fifteen minutes search, the sponge was found and removed.

What are the consequences of this accident? 1. Prompt discovery that a sponge is missing and reopening of the wound and removal of the sponge. When this has been accomplished within a few hours after the operation, recovery without serious consequences from the accident is the rule, though of course the secondary opening of the cavity adds considerably to the danger of the operation.

2. Peritonitis and death. Even though sterile, a large foreign body in the peritoneal cavity, following a severe operation, may invite the localization of bacteria with resulting peritonitis. If a post-mortem examination is held, the sponge is found. If there is no post-mortem examination, the death is supposed to be due to ordinary peritonitis. Patients have no doubt died from this condition without the true cause of death ever having been surmised. In a series of 29 reported cases in which complete data were given, in two cases the sponge was missed before the abdomen was closed and was searched for and found, in six cases it was found by reopening the wound at a period varying from a short time after operation up to four days after, and in twenty-one cases the sponge was found on post-mortem examination.

3. Walling-off of the sponge, with gradual extrusion toward the surface of the body. A strong wall of adhesions is thrown out around the sponge and this localizes any inflammation resulting from it. An abscess may form about the sponge or the region may show only chronic irritation without pus. Because of persistent disturbance in the vicinity of a drainage opening or elsewhere about the incision, an investigation is made and the sponge discovered and removed—a few days to a few weeks following operation.

4. Walling-off and gradual extrusion from the peritoneal cavity into some hollow organ, usually the intestinal tract. The penetration through the intestinal wall may be effected by inflammatory ulceration from an abscess about the sponge, or it may be effected by pressure atrophy, or it may be due to a combination of these effects. This extrusion by way of the intestinal tract may take place rapidly or slowly. In one recorded case the sponge passed spontaneously twelve years after the operation. After the sponge has completed its gradual transition into the intestinal canal, the tract which it occupied may gradually contract and close by granulation without further trouble. On the other hand, the tract may remain as a pus pocket or a fistula into which fecal material escapes and which finally requires operative treatment.

5. Complete encysting of the sponge. Sterile bodies of even large size may become surrounded by adhesions and remain in the peritoneal cavity indefinitely. It seems to me that the word "encysted" does not exactly express the condition, as there is not necessarily a distinct capsule, but a better word has not occurred to me. The sponge really lies in a mass of granulation tissue which penetrates it in all directions and shades off gradually into the surrounding structures.

Von Bunker, quoted by Schachner, conducted some experiments to determine just what changes took place in this disposition of sponges left in the abdomen. He experimented with sterile sponges soaked in turpentine. Within a few hours the sponge was surrounded with fibrin, which shut it off from the general peritoneal cavity and also filled the spaces of the sponge itself. Gradually leucocytes extended throughout the fibrin, and later granulation tissue penetrated all through the sponge, so that it became imbedded in granulation tissue, which gradually shaded off into the surrounding tissues "without any sharp dividing line." Phagocytes set to work disintegrating the foreign body and removing it piecemeal. Several instances are reported in which sponges, left in the abdomen several years, had largely disappeared by phagocytosis.

Of course this comparatively innocuous disposition of the sponge occurs in only a very small proportion of cases. In most cases in which a sponge is left in the abdomen serious symptoms probably develop sooner or later, causing the patient's death or necessitating a secondary operation.

How may the accident be made impossible? This is the most important question of all, and it is one to which I have given considerable study. Following the usual technique, I operated for years without accident. But about two and a half years ago I left a gauze pad in the abdomen. It was a pus case in which extensive drainage was necessary and, fortunately, the pad was discovered and extracted through the drainage wound. The patient recovered without serious result from the accident, but the lesson was not lost. I determined to find some method that would really prevent such an accident—a method that would be entirely under control of the operator and first assistant (a greater division

of responsibility increases the danger) and one that would occasion no delay in the closing steps of the operation.

There had to be taken into consideration the large pads, for holding the intestines out of the way, and the small pads and gauze pieces for sponging.

In place of the several large pads for packing back the intestines, I adopted the large roll of gauze, then already in use by a number of operators, and found it entirely satisfactory.

The matter of the small pads and sponges, however, was not so easily disposed of. I felt that it was imperative to find some method that would do away entirely with dependence on the counting of the sponges at the close of the operation. As long as there was dependence on counting of the numerous small pads and sponges, there would be mistakes, and consequently sponges would occasionally be left in the cavity.

To eliminate this hazardous dependence on counting and to provide a method that would make the leaving of a sponge in the abdomen practically impossible, was not an easy task. I worked over the problem for the greater part of a year. I tried various methods in common use for keeping track of the small pads and sponges, such as clamping an artery forceps to a tape attached to each sponge, attaching a heavy ring to each tape before sterilization, clamping each tape or a corner of each sponge to the sterile sheet about the wound, etc. But I found no such method that was convenient and absolutely safe.

It then became evident to me that if safety were to be secured, the detached pads and sponges must be eliminated entirely. In pursuance of that idea I devised the method published in detail last September.¹

The underlying principle of this method is that no detached piece of gauze shall enter the abdominal cavity. Each piece of gauze introduced is simply part of a very long piece, the greater part of which is always outside the cavity.

I use long folded strips of gauze, each strip packed in a small cloth bag in such a way that it can be pulled out a little at a time as needed. I have used the strips systematically in place of all small pads and sponges. So far as I know the method is original, no description of such use having come to my notice. The nearest approach to it that I have come across, is the recommendation of some writers that a part of every abdominal pad and sponge should always be kept outside the cavity. For some years the large roll of gauze for packing back the intestines has been used by many operators, also gauze strips of various widths and lengths (including five-yard and ten-yard lengths) have been in general use in abdominal surgery for tamponade to check bleeding. But that is very different from the method here detailed of using long strips systematically so as to eliminate all detached pads and sponges.

It is the packing of each long strip into a bag that makes this use of

¹Interstate Medical Journal, Vol. XIV., No. 9. In that article the method is fully illustrated.

strip-gauze practical and convenient. The small cloth bag confines the long strip in a small space so it is not in the way.

Each strip consists of a piece of gauze ten yards long and a half a yard wide. This is folded lengthwise so as to make six thicknesses. The folded strip is approximately three inches wide and ten yards long, with the raw edges turned in and the ends tacked with thread to keep it from unfolding. Each bag is five inches wide and ten inches deep, and is preferably made of extra heavy material and is sewed in such a way that there is no chance for a raveling to be pulled out with the gauze.

Beginning with one end, the gauze strip is packed firmly, a little at a time, into the bag. When packed in in this way it comes out readily, a little at a time, as pulled out. When the end of the strip is introduced to the bottom of the bag, it is well to fasten it firmly there by stitching through and through, so that if by any possibility the whole strip should be packed into the abdomen (to check a sudden severe hemorrhage or for other reason, the end would still remain securely fastened outside. When all the strip has been packed into the bag the top of the bag is closed by folding over and is pinned with a large safety pin. Another large safety pin is also attached to the bottom of the bag. Four of these filled bags, constituting one set, are wrapped together in a cloth and are then ready for sterilization.

Beside the operating table, they are placed in a dry basin, close to the basin containing the gauze roll in normal saline solution.

At the operation, the lower end and upper end of the bag are pinned to the sterile sheet, a sufficient distance away to bring the mouth of the bag conveniently near the wound but not in the way. The gauze strip is used as a sponge by catching a small part of it with the fingers or with forceps and pulling it out of the bag as required and then sponging in the abdomen. After use, this part is dropped away from the wound and another small part is drawn out and used. The used part is *not* cut off but simply dropped outside the operative field and, as more and more of the strip is used, this soiled part falls off the table and out of the way. Thus the greater part of the strip is always outside the abdominal cavity. No detached pieces of gauze are used in the cavity, and hence none can be left there.

Usually two strips, one placed on each side at the beginning of the operation, are used in the course of an ordinary abdominal section. In cases where there is but little sponging, only one strip is needed. In very extensive operations where an extra amount of sponging is required, three or four strips may be needed. In no case did I find it necessary to use more gauze than that contained in one set, though I always have an extra set sterilized and ready for use. I tried different lengths and widths of strips, and prefer the size here given.

For sponging I use these strips exclusively from the time the skin is incised until the peritoneal cavity is closed. At first I anticipated con-

siderable tangling of the gauze strips about the forceps in the wound, but found that this could be easily avoided by always dropping the soiled portion of the strip outside the field *close to the bag*. This prevents the accumulation of loose folds about the wound, with which the instruments may become entangled.

Having used this method now for a year and a half, in various kinds of abdominal cases and under differing environments, I feel justified in recommending it as safe, practical and convenient. Combined with the use of the large roll of gauze for packing the intestines away, it simplifies the matter of pads and sponges for abdominal section and eliminates entirely the chance of leaving a piece of gauze in the abdomen.

Special points. Before passing to the consideration of instruments left in the abdomen, it will be profitable, I think, to take up briefly some special points in regard to the use of the gauze-strip sponges and some possible criticisms that have been brought out in my study of the subject or in questions asked me by surgeons contemplating the use of the method.

In the first place the object of this method is not convenience, but *safety*. Its existence depends solely upon the desire to eliminate every chance of leaving a piece of gauze in the abdomen. Incidentally, I have developed the method in such a way that it is convenient—in some particulars more convenient than the ordinary detached sponges. But this convenience is only incidental.

I call particular attention to this point for the reason that the simplicity of the method and its convenience in certain particulars (ease of preparation, compactness, sponge always within reach of both operator and assistant), have caused some to jump to the conclusion that its simplicity and convenience constitute the reason for its promulgation. That is a mistake. The time and study required for the development of this method, were given only because of the pressing necessity of finding some universally applicable method that would make practically impossible the serious accident of leaving a sponge in the peritoneal cavity. The pads and sponges commonly used in abdominal work are fairly convenient. On that score no decided objection can be laid against them—certainly none of sufficient weight to justify the radical change here contemplated from the long-tried and generally employed technique of abdominal surgery.

The fatal drawback to the ordinary pads and sponges is the danger of one being left behind. If this were only a theoretical danger, I would not take your time considering it. But facts previously given show that many lives have been sacrificed and are still being sacrificed by this accident. Think of a series of 155 reported cases in which it was recognized that a foreign body had been left in the abdomen—about 70 per cent. being sponges. This undoubtedly represents only a small proportion of the recognized cases, being made up from reports of the work of comparatively few surgeons and those the more prominent and ex-

perienced ones, i. e. the ones with whom the accident is least likely to occur. The bulk of the recognized cases are probably never reported, for the accident would naturally be kept quiet unless there were some special reason for reporting it. Again, think of the cases that are not only not reported but that are never even recognized. The patient dies with evidence of peritonitis, there is no suspicion of anything unusual, no post-mortem examination is made and the death is supposed to be due to ordinary peritonitis. The possibilities in this direction are indicated by the fact, previously mentioned, that in a series of 29 cases, in which complete data are given, in 21 of the cases the accident was recognized only on post-mortem examination, when the sponge was found.

Of course, where a surgeon always operates in the same hospital with the same assistants and to a large extent with the same nurses month after month, the danger is reduced to a minimum, because of the establishment of a routine from which there is almost no departure. Even under these circumstances, however, the danger is not entirely eliminated. And what of the great bulk of surgical work, where the operator works at different hospitals, with different nurses and in some cases with changing assistants? Furthermore, the progress of the operation is not always smooth and regular. Abdominal surgery is notably full of uncertainties and unlooked-for developments and trying situations, that break the routine of the best regulated institutions and tax to the utmost the ability and steadiness and attention of all concerned in the operation. How easy it is for a slip to occur under these conditions. And, still further, think of the emergency work, in unsuitable environment and with untrained assistants. In estimating the possibility of this accident, all these conditions must be taken into consideration. Likewise, all these conditions had to be considered in devising a method for preventing the accident. To be worthy of general use the method must be absolutely safe under all these varied conditions.

Hitherto there has not been a method, practically applicable in all the vicissitudes of abdominal surgery, which would absolutely prevent this accident. In spite of widespread interest in this subject in recent years and of considerable study and investigation of it and several excellent papers by different authorities, there has been no signal advance. Ten years ago operators were using the same preventative measures now commonly employed. The sponges were counted, tapes were attached to the sponges that were counted, forceps were attached to the tapes that were attached to the sponges that were counted, etc., etc. Yet with all these complicated precautions, many sponges were still left in the cavity as the records show. In fact in one reported case the sponge, tape, forceps and all were lost in the abdomen, indicating the uncertainty of that method.

I feel now, however, that we have a really safe method, in the simple and convenient one here detailed. I did not arrive at this conclusion hastily but tried the method a full year and under various circumstances,

before recommending it. I have used it now for a year and a half as the routine and only method in various classes of abdominal-section cases and in differing environment, and the longer I use it the better I like it. Even if it were decidedly less convenient than the regular pads and sponges, I should consider its use imperative because of its safety. The greater part of the strip is always outside the abdomen and if, by any possibility, the whole strip, ten yards in length, should be hastily packed into the abdomen to check oozing, the end would still remain out, for it is fastened securely to the bag and the bag to the sterile sheet. I do not see how there is any practical possibility of a piece of gauze being left in the abdomen, even in the most trying case and with wholly untrained assistants. The hazardous dependence on the final counting, or on watching what goes in and what comes out of the cavity, is entirely eliminated.

In this connection I wish to call attention to a rule which should be most strictly observed, viz., *never cut a gauze-strip sponge in the course of an operation*. The temptation to cut the strip comes not infrequently, because in certain situations it makes the sponging somewhat more convenient. In some situations the cutting would of course not be dangerous, as when part of the strip outside is cut off and allowed to drop away. On the other hand, in other situations the cutting of the strip might lead to a portion being left, as when a part is used for temporary packing and then the strip is cut in order to sponge more conveniently with the remainder. Whenever a cut is made in one situation for any reason, the rule is broken, and then a cut is likely to be made on the spur of the moment in any other situation where it appears to increase the convenience—and thus absolute security is lost. The only safe plan is to adhere strictly to the rule never to cut a strip during the course of an operation. Of course, if at the close of an operation it is desired to use part of a strip for permanent packing or drainage, that is a different proposition.

Another question that has been put to me, is as to the size of the strips. I experimented with different sizes. Those used at first were much narrower. When such a narrow strip is wet with blood it becomes like a ribbon—not enough substance in it to sponge well. Of the various sizes tried, I found ten yard strips, half a yard wide, the most convenient. Folded as indicated such a strip is narrow enough for use when a very small sponge is required, while on the other hand several folds caught in the forceps, furnish the substance for a large sponge. Also, it can be easily spread out sufficiently to wall-off an object with sheet-gauze, as, for example, in surrounding the region of the appendix when that structure is to be removed. When the gauze used in making the strips is extraordinarily thin, the width should be doubled.

I have been asked about the cost of this method. Preventing, as it does, one of the serious accidents of abdominal surgery, it is cheap at any price. Even though its use cost several times as much as the dangerous

detached sponges, that would not constitute a valid objection. As a matter of fact, however, it costs no more than the usual method—if any difference, the cost is somewhat less. In order to get definite information on this point, I ascertained the amount of gauze generally used in an ordinary abdominal section in each of four of our leading hospitals. Though the number and size of the pads and sponges differed greatly in the different institutions, there was a striking uniformity in the amount of gauze consumed in an ordinary abdominal section—averaging 20 to 25 yards in each institution. The amount ordinarily used in the method which I have detailed is 15 yards—the 5 yard roll for packing back the intestines and 10 yards in the two gauze strips in bags. In severe cases the third gauze strip is used. I do not recall a case in which I have had to use more gauze than this, but even if the whole set were used, it would not run over the amount consumed by the usual method.

I have been asked if the strip extending from the forceps to the bag is not in the way when sponging? Sometimes it is to a slight extent, but not as much as would at first appear. Any new method seems somewhat awkward at first and this is no exception to the rule. However, in my experience so far, I have not found any situation in which there was serious interference with satisfactory sponging or with any other operative manipulation. Like any other important step in technique, it should be studied until it is clearly understood before an attempt is made to use it. There are two particular points that may be mentioned. To prevent the accumulation of loose folds of gauze in the vicinity of the wound, with consequent entangling of the instruments, the used portion of the strip should always be dropped outside the field but close to the bag. Again, when taking hold of a fold, to sponge with, draw it out of the bag for some distance, so that it can be introduced into the abdomen as far as desired freely and without tension.

I have been asked if, in using this method, it is necessary to take one's own sponges to the different hospitals. Not at all. Where the operating-room nurse is not familiar with the method, she is given, a day or two before the operation, a slip containing definite directions for preparing the rolls and strips.

Nurses, as a rule, welcome the method, stating that it is far less troublesome than the sewing of the numerous small pads and sponges. The directions are as follows:

ROLLS AND STRIPS FOR ABDOMINAL SECTION.

- 1 roll of gauze—5 yds. long, 9 in. wide—4 thicknesses.
- 4 strips of gauze—10 yds. long, 3 in. wide—6 thicknesses.
- Have another set (one roll and four strips) in reserve.

For the rolls, yard-width of gauze is used—when folded to form four thicknesses it is 9 in. wide. For the strips, the yard-width of gauze is divided into two strips and each of these, when folded to six thicknesses, is about 3 in. wide.

Turn in all raw edges so that no raveling can be left in the abdominal cavity.

For the Roll, fold back and forth instead of rolling in the ordinary way, so that it can be spread out quickly. Wrap the roll in a cloth for sterilizing.

Pack each Strip into a separate small cloth bag (5 in. wide and 10 in. deep) and attach a large safety pin to the bottom of the bag. Make the bag of firm extra-heavy muslin or drilling and sew with French seams to avoid ravelings on the inside. The end of the strip first introduced to bottom of the bag should be fastened there securely by stitching through and through. Then pack the strip firmly into the bag in such a way that it will come out easily, a little at a time as needed. When filled, fold over the top of the bag and pin with a large safety pin. Wrap together in a cloth, each set of four strips (each strip in its bag). Sterilize in the usual way.

At the time of the operation, a convenient arrangement, particularly for emergency work, is to place two sterile basins within reach of the operator. One basin is to be partly filled with hot normal saline solution and is for the roll of gauze, while the other basin is to hold the four bags containing the gauze strips which are used dry.

That is all the paraphernalia necessary, and the "pads and sponges" are all within instant reach of the operator. The advantage of having the sponges always within instant reach of the operator will be particularly appreciated by those who have been obliged to handle serious and troublesome intra-abdominal conditions without trained assistants.

The gauze strips may be used also for temporary packing to check hemorrhage or for any other purpose for which strip-gauze may be required in the course of an operation.

INSTRUMENTS.

In about 25 per cent. of the reported cases in which a foreign body was left in the abdomen, the foreign body was an artery forceps. In a series of 19 cases, seven of the patients died. In another case in this series, the forceps was missed before the wound was closed and was searched for and found in the cul-de-sac. In two cases the forceps was missed after the abdomen was closed and the wound was reopened and the forceps found. In two cases the forceps was removed from an abdominal abscess, in one case the forceps passed into the bladder and in three cases the forceps passed spontaneously per rectum in nine months, ten months and four years respectively after the operation.

To prevent leaving any foreign body in the abdominal cavity, it is necessary to take into consideration everything used in the wound or about it. When considering this subject two years ago, it became evident to me that dependence on the counting of instruments at the close of an operation was just as hazardous as the dependence on the counting of sponges. So I decided to eliminate this hazardous dependence on counting by the use of very long instruments exclusively, as previously recommended by some authorities.

Every instrument used about the wound is long—so long that a por-

tion of it is practically always outside the abdominal cavity. Again, if by accident such an instrument should slip entirely into the cavity, its length is such that it would almost certainly be felt when the hand is carried into the cavity for the final palpation before closing. All the artery-forceps, dissecting-forceps, tenaculum-forceps, pedicle needles, scissors and other instruments for internal work are between seven and eight inches long, the shortest being the needle holders (7 to 7½ inches). The shortest instruments used anywhere about the wound are the heavy abdominal-incision scissors and the scalpel (6 to 6½ inches), both of which are laid aside as soon as the peritoneal cavity is open.

The needles and Murphy buttons are not brought near the wound except when held in a forceps or with a suture attached. No Michel clamps (for holding rubber tissue or gauze along the wound margin) or other small unattached objects are allowed near the wound as long as the peritoneal cavity is open.

MEDICO-LEGAL COMPLICATIONS.

It is obvious that foreign bodies left in the peritoneal cavity during operation are very likely to lead to medico-legal complications. Several suits have been filed as a result of such accidents. But, so far as I have noticed, there has been no award of damages and, the theoretical questions as to what amount of legal responsibility attaches in such a case and to whom it attaches, have never been thoroughly threshed out. There are usually other conditions and questions that tend to obscure the issue. Each case had modifying features of its own.

It seems that the circumstances of each case must determine whether in that case the legal risk of such an accident should be assumed by the surgeon who does the operation or by the nurses who care for the sponges or by the institution in which the operation takes place or by the patient. The patient herself must assume some risk in every operation and she certainly can not cast on others all the risk due to unusual and unforeseen accidents, where reasonable skill and care are exercised.

Probably the most celebrated medico-legal case of this kind was that in which suit was brought against Prof. Kosinski. A synopsis of this famous case is given in Schachner's article. The principal facts were in brief as follows: On December 22, 1897, Kosinski operated for the removal of an ovarian cystoma in a patient 50 years of age. It was a complicated case and the operation was very difficult. After a few days there was an elevation of temperature and some pains and, in the examination, an inflammatory infiltration could be felt near the pelvic brim. In the meantime two artery forceps had been missed. It was thought that the missing forceps might be in the abdominal cavity and, after six weeks, the suspicion became so strong that the abdomen was reopened. The suspicious mass of exudate was investigated but no forceps were found. After this the patient became better and left the hospital.

The trouble persisted, however, and three months later Kosinski still

thought the forceps might be in the abdomen. He communicated his suspicions to the family and insisted on another operation and offered to perform it gratis. The family would not consent to another operation. The patient went to several physicians, one after another, hoping to get well without an operation.

Finally in June, 1898, the symptoms became acute and threatening and the physician who was called in, insisted that the patient be taken to Kosinski at once that he might perform the operation, which had then become imperative. This the family refused to do and called in another physician, who operated. On opening into the mass at the pelvic brim he found a cavity in which lay the two artery forceps. Both forceps had forced an entrance into the external iliac artery. The removal of the forceps was attended with a furious hemorrhage, from which the patient died on the table. This was six months after the primary operation.

Suit was entered against Kosinski and there was an extensive trial, which resulted in the acquittal of the accused as far as causing the death of the patient was concerned—it having been shown that he strongly insisted on a line of treatment which would probably have prevented the patient's death, had the treatment not been peremptorily rejected by the family.

A curious feature of this case was that a number of radiographs of the suspicious area were made, but not one of them showed the forceps—the failure being due doubtless to defective technique.

EXAMINATIONS OF THE FECES AS A ROUTINE PROCEDURE.

By JESSE S. MYER, M. D., of St. Louis, Mo.

Today the clinical laboratory does not present a more interesting chapter nor a more fertile field for work than the examination of the feces. As a routine procedure this work has taken its place in the same rank with the examinations of the urine, stomach contents and blood. In many diseases of the gastrointestinal tract, as well as certain diseases manifesting themselves only in constitutional symptoms, an absolute diagnosis is often impossible without the examination of the feces. It is true that this work has not become as popular, and is not as universally done as are the examinations of other excretions, and in private practice will probably never be so, because of the unpleasant duty on the part of the patient of collecting the specimens and the unpleasant features in the examination. The work cannot be done satisfactorily on the "little table in the corner" because of the unpleasant odor. For this reason, as well as because of the specialized knowledge required, the work will in all probability always be done by those especially prepared for it, both as to laboratory facilities and experience.

Personally, I can say that the satisfactory results derived from a study and practice of the examination of the feces rob the procedure of practically all of its unpleasant features. The intestines are unfortunately so situated and constructed that the physical examination yields but little that is positive, excepting, of course, in case of tumefactions, stenoses and localized inflammations. In the diffuse affections of the intestines, the physical examination reveals practically nothing aside from the usual areas of tenderness. The character of symptoms produced by the various diseases of the intestines are so similar in nature that we are justified in saying that there are no pathognomonic signs of certain pathological lesions. The previous history, while helpful in arriving at a diagnosis, does not make possible a positive diagnosis except in isolated cases.

Until the introduction of the stomach tube and the test meal, within the past two decades, the diagnosis of gastric disturbances was, for the same reasons, not reduced to a science as it is at the present time. Since the development of this art, the examination of the stomach contents has become a general practice among physicians who have the time and inclination to call to their aid laboratory findings in the diagnosis of gastric disturbances. No one can dispute the great value that is attached to these examinations. The diagnoses of intes-

tinal diseases are quite as amenable to laboratory interpretation, and yet only during the last decade has the subject received anything like its merited attention and then only from a few laboratory workers and specialists in gastrointestinal diseases. While it is a fact that experience and persistence are of the greatest importance in the development of thorough knowledge of the technique, nevertheless with reasonable powers of observation and even a meagre knowledge of microscopy and chemistry, much can be accomplished. Because of the great variety of substances ingested and consequently excreted in the feces, the work presents a most fertile field for misinterpretation. Therefore, repetition, persistence and careful observation are the chief requisites.

The objects of the examination of the feces may be said to be three-fold, namely: (1) The diagnosis of diseases manifesting themselves through symptoms on the part of the gastrointestinal tract in the largest sense of the term, including therein the stomach, intestines, liver and pancreas. Among these symptoms may be named diarrhea, dysentery, colics, constipation, mucus and blood in the stools, etc. (2) Certain obscure constitutional diseases (pernicious and secondary anemias, convulsions in children and certain nervous manifestations, etc.,) in which there may or may not be accompanying symptoms on the part of the gastrointestinal tract. (3) The functional determinations of gastrointestinal digestion.

It shall not be within the scope of this paper to enter into the discussion of the details of technique of examinations of the feces, for this would involve, depending upon the point to which these analyses are carried, the most intricate problems of chemistry and bacteriology. In the ordinary routine work, these are not required any more than are the examinations for indican, acetone, the diazo reaction, etc., in the routine examinations of the urine.

In order to draw deductions from the microscopic examination, which is by no means the least important part of the examinations, the stool should be obtained in as fresh and undisturbed a state as possible. It should be inspected in a fresh state, because upon standing the surface of the stool becomes darker through the further oxidation of the coloring matter, and also because of the destruction of the motility of certain parasites, such as the ameba coli, etc. The stools should be passed if possible into the very vessel in which it is brought to the laboratory, (a Mason jar will suffice) in order that blood, mucus, etc., may be observed in its relation to the entire mass of feces. In the inspection of the feces, there must be taken into account the amount, consistency, color, odor and the presence of macroscopical particles. As to the amount of feces passed in twenty-four hours, but little need be said, for this varies with the quantity and quality of food ingested. One partaking freely of proteids, such as meat, eggs, etc., which leave but little residue, will necessarily pass a smaller, harder, darker stool than

one eating freely of carbohydrates, in the form of potato, spinach, asparagus, and the green vegetables in general. These substances leave a large residue of cellulose. For this reason the season of the year may greatly influence the size of the daily passages, the stools being larger in summer, when one eats freely of fruits, berries and green vegetables. Copious, infrequent stools point, as a rule, to the existence of a disturbance in the upper portion of the intestine, while meagre, frequent stools point to the existence of the trouble in the lower portion of the intestine. While one or two fairly copious stools daily may be considered the normal state, there are many cases on record in which individuals, in a perfect state of health, are uncomfortable without three or four actions daily; while on the other hand there are certain individuals who find that they feel best when having but one action a week. These, however, are exceptions to the general rule.

The normal consistency of the feces, while difficult to describe in words, is a point to which physicians have called attention for many decades as an indication of the state of the intestinal tract. The stool should be of such a consistency as to maintain its form after being passed without, however, retaining the imprints of the haustræ. A liquid stool is always abnormal if not produced by a cathartic. The normal stool then should be a soft, cylindrical mass or form in the vessel a cone-shaped or pyramidal mass. Hard scybalæ indicate a prolonged retention in the colon, for it is in the colon that the water is extracted from the intestinal contents. A diarrhetic stool indicates an increased peristalsis on the part of the colon alone, or the small intestine and colon. The contents enter the colon in a fluid state and, therefore, if hastened through the colon are expelled in this state.

The color of the feces is usually dark brown, due to the end results of bilirubin oxidation to urobilinogen. In total absence of the bilirubin from the intestinal tract, as in cases of complete obstruction of the common duct, the stool has an ashen gray or clay color. A number of variations in the color of the normal stool may be produced by the character of food eaten; if meat predominates the color will be a dark brown; if vegetables, a light brown; if milk, a light or orange yellow; and if fat, a grayish yellow color. Vegetables containing iron,—such as spinach,—give a darker color to the feces. The influence of the various drugs upon the color of the feces is well known. Heavy metal salts, such as bismuth subnitrate, iron preparations, etc., produce a black color as a result of their conversion to bismuth or iron oxydide. In these cases the microscopic examination shows the color to be due to rhomboid-shaped black crystals. Charcoal also gives a black color to the stool and is found in the feces microscopically as amorphous black masses. Therefore before venturing a diagnosis as to the presence of blood in the stools, the patient should be carefully questioned both with reference to drugs and food taken in the previous forty-eight hours. These factors have often led to the mistake of suspecting blood, which

gives a similar color to the stool. Depending upon the amount of blood present and the source of its origin, the feces may vary from a very dark brown to an inky black, due to the conversion of hemaglobin to hematin. If the stool appears black in color, due to blood, it indicates that the blood has been in the intestine for a considerable time, hence, comes from the upper part of the gastrointestinal tract, as, for instance, in ulcer of the stomach and duodenum. If, on the other hand, the blood comes from the lower portion of the tract, it is more apt to appear in the fresh state, giving to the stool a reddish brown color. While this is the rule, it is not infallible, for it must be remembered that blood coming from the small intestines may be carried through so rapidly as to appear in the fresh state, and on the other hand, blood coming from the colon may remain there so long as to be changed in the colon. However, if the stool is black through and through and is passed in a formed state, we may conclude that it was completely admixed with the feces while still in a liquid state, and inasmuch as the feces begins to harden in the colon, such an admixture must have taken place in or above the cecum. Fresh blood intimately admixed with the feces occurs only in liquid stools and usually comes from the colon. If the stool is formed, fresh blood is not found thoroughly admixed with it, for reasons that are evident. A few drops of fresh blood passed immediately after, hence on top of the stool, come invariably from the rectum (hemorrhoids, fissures, etc.), and the same may be said of a drop or few drops immediately preceding the stool. Blood adhering to the surface of scybalous masses come usually from a point below the splenic flexure of the colon, most often from the sigmoid, because it is here that the formed stool takes on the shape of the haustræ.

In my experience these same rules may be applied to mucus in the stools. Small bits of mucus intimately admixed with liquid feces usually come from the small intestine, while the larger masses not intimately admixed with the feces usually come from the colon. Much depends upon the length of time that was required for a given stool to be expelled, as to character of mucus found. It has been maintained by some authorities that mucus from the small intestine is practically never found in the feces, having been dissolved in its transit. Mucus on the external surface of scybalæ comes from the lower portion of the colon, almost invariably the sigmoid or upper portion of the rectum. Small quantities of jelly-like mucus passed just before or just after the stool, come from the sigmoid or rectum.

After the stool has been inspected with reference to the qualities just described, a portion should be set aside for microscopic and any chemical examinations that may be indicated. The remainder is washed through a specially designed sieve, and the large particles remaining on the sieve are examined with reference to (1) food remnants, (2) parasites, (3) concretions, (4) foreign bodies. There occur in the feces of practically every individual partaking of a mixed diet,

a certain amount of food residue, which varies in amount according to the quantity and character of food eaten. The greater the amount of vegetables, the greater the residue. This is due to the fact that practically all vegetables contain a greater or less amount of cellulose, which is practically indigestible in the human intestine. The vegetable residue that is to be found in any stool depends upon three factors: (1) The amount and character of the cellulose present (capsules of peas, and beans, the fibro-vascular bundles in celery and the stalks of vegetables generally, the seeds of berries and fruits, etc.). (2) The mode of preparation of the food (as to whether raw or cooked, strained or unstrained, etc.). (3) Degree of mastication.

There is no doubt but that much of the indigestion, gastric and intestinal, is due to the poor preparation and to the rapid bolting of food. It is amazing at times to note the large amount of undigested residue which may be found in the washings of the feces, not only of children, but of unthinking adults as well. Large quantities of raw fruits with seeds, the stalks of vegetables, peas, beans, corn, etc., are swallowed without being properly masticated, and often without being properly cooked, and as a result are found practically unchanged in the feces. I have often found a demonstration of this sort a splendid object lesson to patients and have thus often succeeded in getting them to correct their habits by showing to them the probable cause of much of their indigestion. I could cite a number of cases in which a change in the character of food and habits have completely corrected the existing gastrointestinal disturbances. Unless meat is swallowed in large portions, it is rarely found in the stool of a healthy individual, excepting as microscopical remnants of muscle fibres. Even a large bolus of meat is usually broken up before it is evacuated with the feces. However, in cases where diarrhea already exists, meat may be found in the feces in a practically undigested state. Schmidt has shown that the fibrous tissue is digested in the stomach, while the muscle fibres are largely digested in the intestine. Therefore, a poor digestion of fibrous tissue indicates a gastric disturbance, while the poor digestion of the muscle fibre indicates an intestinal indigestion; a poor digestion of both indicates a disturbance in the stomach and intestines.

Not infrequently intestinal parasites, or portions thereof, are found in the coarse washings. The parasites which we most frequently meet in this section are *tænia saginata* and *tænia solium*, or tapeworms obtained from eating raw beef and pork, respectively, *ascaris lumbricoides*, or ordinary round worm, the *oxyuris vermicularis*, or pin worm. In the last decade another parasite has been added to those sometimes met with even in this state, namely, *ankylostomum duodenale* or hook worm. This is essentially a tropical disease, which of late years has migrated to this country, and not infrequently cases are reported in various sections of the United States, especially in the Southern States. The worms, however, are not themselves usually discovered in

the feces, but are detected by the presence of the eggs, of which I shall have occasion to speak later. Often the first evidence that one has of the existence of parasites is in the discovery of segments of the tapeworm, of the male or female ascaris, or of large numbers of the oxyuris. These are practically the only parasites that we meet with in this section visible to the naked eye, and the diagnosis may be promptly made upon discovering portions or the whole parasite.

The concretions that may be met with in the washings of the feces are gall stones, fecal concretions and fatty acid concretions. Gall stones are often searched for after acute attacks of colic in which cholelithiasis is suspected. In such cases the feces should be examined for a period of at least seventy-two hours following the attacks. While the inability to find the concretion in the stool does not exclude the existence of gall stones, the positive finding absolutely confirms the diagnosis. When a gall stone passes through the common duct into the duodenum, it is usually found intact in the feces, though it has been shown, especially in experiments upon animals, that they are sometimes crushed and destroyed in their passage through the bowels. Fatty acid concretions, so often found in the feces in those ingesting large amounts of oil, especially olive oil, have led to the employment of this method by quacks to deceive the layman. Persons afflicted with paroxysmal attacks of pain in the abdomen are given large quantities of olive oil, some of which the intestine is incapable of assimilating and absorbing, and is, therefore, expelled in the form of fatty acid concretions. These concretions, are sometimes passed by the handful and are demonstrated to the patient as the sought-for gall stones. If one is in doubt as to the character of a concretion, i. e. whether it is a fatty acid, fecal concretion, or a gall stone, a section through it will usually enable one to say definitely to which class it belongs. If there is still doubt, the concretion may be dropped into ether, which, in the case of a fecal concretion, will have no effect at all; but in the case of fatty acid concretions or gall stones, will dissolve them. After the evaporation of the filtered ether, cholesterin will be left behind as a precipitate if the concretion is a gall stone, while only fat droplets will remain in the case of a fatty acid concretion. Fecal concretions may be at times so hard as to greatly resemble gall stones in appearance and structure. In this case the chemical test will very quickly clear up the diagnosis.

It would be impossible to include within the scope of this paper the details of the chemical, microscopic and bacteriological examination of the feces. The microscopic examination presents by far the most interesting phase of the subject, because of the great variety of vegetable cells and other morphotic elements. Through the aid of the microscope one recognizes the microscopic varieties of intestinal parasites, which escape, of course, the macroscopic examination. Among these may be included ameba coli, with which we now meet not infrequently in this state. Since the Spanish-American war, especially, a number of cases have been reported. Formerly the

disease was essentially a tropical disease and found only in seaport towns in this country, but in the last ten years especially cases have been reported from practically every section of the country and as far north as Michigan. It was my opportunity to report one of the first cases, if not the first case officially reported in this state, in 1900. The moving amebæ are to be found in the sanguinous mucus on a slightly warmed stage. An absolute diagnosis cannot be definitely made unless they are found in motion. Among other unicellular organisms found in the feces may be mentioned the *cercomomas intestinalis*, the *trichomomas intestinalis*, the *lamblia intestinalis*, etc. These have no recognized pathological significance and need only be mentioned in passing.

The eggs of the various parasites may be recognized in the microscopical examination of the feces and thus a definite diagnosis of the presence of the worm made. Only recently a case came into my hands in which I was able to make a diagnosis of double intestinal parasitism (*trichocephalus dispar* and *ascaris lumbricoides*) through the presence of the eggs. In hook worm disease, the worm itself is rarely found, and the diagnosis can only be definitely made on the demonstration of the eggs, which are quite characteristic. It is scarcely possible to make a differential diagnosis, from the appearance of the eggs alone, between the *tænia solium* and *tænia saginata*. But, with this exception, there is little resemblance between the eggs of the various intestinal parasites. Upon finding the eggs, therefore, the character of the parasites present may be readily determined. Great care is necessary not to confound the eggs with remnants of vegetable cells, which to the unpracticed eye may appear very similar. Plant hairs, plant spirals, various forms of vegetable cells, epithelial cells, blood corpuscles, leucocytes, various bacteria, fungi, etc., all play a role in the microscopical examination of the feces. Among the crystals found may be mentioned fatty acid, oxalate of calcium, ammonio-magnesium-phosphate, and the Charcot-Leyden crystals. It will be impossible here to dilate upon the significance of the microscopical findings of all of these morphotic elements, and one cannot hope in the allotted time to do more than call attention to them.

The test diet of Schmidt and Strassburger has received much attention during the past few years from those engaged in this sort of work. A diet of known consistency is administered to the patient for several days and the feces examined as heretofore stated, with reference to food remnants, the stage of digestion, the presence of fibrous tissue (indicating a deranged gastric digestion), undigested muscle fibres (indicating a deranged intestinal digestion), the presence of fats, fermentation, bilirubin oxidation, etc. Too much credit cannot be accorded them for the character of their work. Their test diet will occupy the same position with reference to the examination of the feces, that the test meal does in the examination of the stomach contents, and promises just as satisfactory results.

In reviewing the significance of blood in the feces, I purposely omitted until this time reference to the presence of traces of blood in the stool, which do not manifest themselves microscopically or macroscopically. When a very small quantity of blood is present in the stool and has been completely broken up, its presence can only be determined through chemical tests. The presence of minute quantities of blood has been called "occult hemorrhage" and may come from bleeding vessels or the surfaces of denuded areas anywhere along the gastrointestinal tract from the mouth to the rectum. In all cases, therefore, in which ulcer of the stomach, cancer of the stomach, ulcer of the duodenum, or other portions of the small or large intestine, and carcinoma of the intestine are suspected, no examination of the feces is complete without a chemical test for blood by the guaiac-turpentine or benzidine tests. In order to exclude the possibility of the patients having ingested anything containing blood pigments, such as meats, broths, etc., they should be put on a diet free from these substances for two or three days prior to the examination. The mouth should be carefully examined to exclude the possibility of minute quantities of blood having been swallowed, and the rectum to exclude the possibility of hemorrhoids, fissures, etc., (in which case the blood would be seen macroscopically). With these precautions, the finding of a positive trace of blood in the feces, together with the symptomatology and physical findings enable one to arrive definitely at a diagnosis of ulcer or carcinoma, which might otherwise remain in doubt.

The determination of traces of blood in the feces by the guaiac-turpentine and benzidine tests, recommended by Weber and the Adlers respectively, and perfected by Boas, Schlesinger and Holst, and others, has proven one of the most valuable aids, in the diagnosis and differential diagnosis of gastrointestinal diseases, that has come to us in the last decade.

In presenting this résumé of important points in the examination of the feces, it has not been my endeavor to cover the subject, but simply to touch upon those features that enter into the routine examination. The chief object, however, is to direct attention to a class of work that is being greatly neglected by the general profession.

3894 Washington Blvd.

PROGRESSIVE HEMIPLEGIA OF THE LEFT SIDE DUE TO
GLIOSIS AND VASCULAR LESIONS OF
THE RIGHT CENTRUM.

By FRANK R. FRY, M. D. and MALVERN B. CLOPTON, M. D., of St. Louis.

Theo. H., aged 52 years, farmer, large strong man, previous health good.

He was first examined by us November 29th, 1907. In May (six months before this examination) he noticed for the first time a slight twitching in the left cheek, and later in the lower part of the face. This was accompanied by a slight paresthesia, formication and numbness. In the latter part of June or early part of July he noticed that the hand and fore arm were becoming slowly weakened. The spasm in the face continued and a similar one began to appear in the thumb and index finger, and later in other portions of the hand. There was also a good deal of paresthesia in the hand similar to that in the face. The times of appearance of these various symptoms were confirmed by his wife and son, who accompanied him. She stated that in September he began to look poorly and to show signs of general weakness. But he complained of no symptoms except those mentioned above. There had been no vomiting, and very little if any vertigo and no headache. When asked about the latter he admitted that within the last days he had felt for the first time some slight pains in the right parietal region. He was a person of very good intelligence. His mind was perfectly clear. There was no tinnitus or complaint about hearing.

A physical examination showed no tenderness of the scalp or skull and no difference of percussion sounds between the two sides. The pupils were equal and reacted to light and accommodation. There were no complaints of visual disturbance of any kind. The fundi oculi were practically normal.

In the left hand and wrist muscular power was much reduced. There was present a feeble flexion of the wrist and the fingers. Extension of the same was even more feeble, and slow of accomplishment. Rotation of the wrist and the elbow movements were somewhat stronger, but evidently feeble. The shoulder was still stronger but below normal.

There had been some jerking, in the left foot, but no spasm like that of the face and hand. There was probably some loss of power in the toes and ankle, but by comparison with the other side it was difficult to demonstrate it during the examination. He stated that in walking the foot did not feel as secure as the right one, that the leg below the knee especially was weaker.

No knee-jerk could be obtained on either side, even by re-enforcement. There was a feeble plantar (flexion) reflex; better on the right. There was no Babinski, Oppenheim, or paradoxical (Gordon).

During the times he was in the office there were two slight spasmodic seizures of the face. They were confined to the cheek and the angle of the mouth. They were of short duration, succeeding waves of spasm of possibly 15 to 30 seconds. They were not attended by mental phenomena of any kind, but they were accompanied by a paresthesia that was slightly uncomfortable.

At the time of this examination we could not detect in the face, hand or foot or elsewhere any objective sensory defects. The examination was carefully conducted. Position and localizing sensibility, pressure, light-touch, pain and temperature were carefully tested everywhere. There was not the slightest degree of asteriognosis in the hand. Although it was so paretic as to make it very difficult and often impossible for him to retain a grasp on the articles given to him, yet he recognized them in a prompt and normal manner; *e. g.*, he could at once distinguish be-

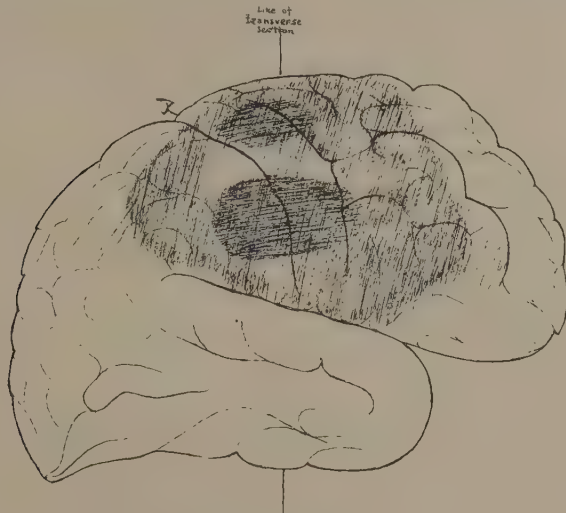


Fig. I. Glioma involving right frontal and parietal lobes. Hemorrhagic areas shaded dark.

tween a quarter piece and a half dollar, naming them correctly. He could quickly tell a camel-hair brush, a lead pencil, a small pocket knife. An iron ring, large enough to slip over the fingers without touching was placed on his finger. He recognized at once what it was, and named correctly the fingers on which it was placed, one after another. All of these tests, of course, were made with the patient's eyes properly blind-folded and the other precautions to make them reliable.

On account of the total absence of sensory signs we concluded that the progressive hemiplegia was probably due to a neoplasm confined to the ascending frontal convolution, or at least not extending posterior to it. We recommended to his physician, Doctor Mark Crawford, of Mexico, Missouri, a prompt exploration.

At this time the "cardinal" symptoms of tumor were absent. He had practically no headache, very little if any actual vertigo, no vomiting and no optic neuritis.

He had a decided thickened condition of the arteries which could be felt in the radials and in the brachials as well. The urine showed nothing important.

We did not see the patient again until January 16th, 1908, seven weeks after the foregoing notes were made.

We learned from his wife and son that within a few days after our previous examination he had had a severe epileptic seizure, which lasted an hour (?). It began with certain rolling movements of the left hand, quickly followed by a general convulsion. Within a week after he had five similar attacks. After an interval of another week he had two more attacks. Two weeks ago, after a very hard attack, the leg became paralyzed and has remained totally helpless since. He had been having headache since his last visit, but it had only become severe within the last two weeks, and was located in the left parietal region. The weakness in the left arm had steadily progressed until the whole member had become totally paralyzed.

We found on examining him that he could not move any portions of the left upper or lower extremities. The lower part of the face was parietic, but relatively not as much so as the extremities. There was a slight knee-jerk and ankle-clonus; a very slight (flexion) plantar reflex; no Babinski, no Oppenheim.

Owing to the patient's stupid condition a satisfactory sensory examination could not be made at this time. It was necessarily limited to contrasting the reaction of the opposite sides of the body to various kinds of stimulation. This was done at times when he was clear enough to respond to some extent, but incapable of sustained attention in a degree necessary for reliable sensory tests. It was evident, however, that there was a good deal of sensibility to pain on the whole left side. He could also at times designate the region pricked or firmly pressed *c. g.* the face and hand.

Three days before he entered the hospital he had had a very severe convulsion (status?), and after it, had been given chloral quite freely. When he reached us it was a question how much of the stupor was due to drugs. The temperature was normal, the pulse 56, the blood pressure 126 (Janeway apparatus), the urine 1023 with 1-10 per cent. of albumin and some hyaline and granular casts.

Next morning (17th) he was much clearer and answered questions coherently. He complained greatly of headache or distress in the right parietal region. From this time until the operation on the following morning he fluctuated between states of profound stupor and partial lucidity. The pulse ranged from 52 to 76, the blood pressure from 106 to 126, the temperature normal. We felt that there was very slight chance of benefit from an operation. But his relatives decided to accept it.

Operation, January 18, 1908. Cushing tourniquet over gauze. A large osteoplastic flap over the right motor area was made by trephining at the angles, cutting on a bevel with a Gigli saw to connect these openings, and making the vertical cuts with a De Vibiss. The base of flap was easily broken and the dura exposed over a space 3 inches in diameter. There was not much bulging. The brain did not pulsate until the dura was opened through a horse-shoe incision $\frac{1}{2}$ inch inside the bony margins. Palpation showed no induration or evidence of a tumor over any of the area exposed. The dura was immediately closed with cat gut. The closure was attended with some difficulty on account of the bulging of the brain. There was marked oozing at the time from the scalp and a temporary dressing was applied. In a few minutes all bleeding had apparently stopped. The permanent dressing was applied without draining the wound. The patient's condition was good throughout the operation. The brain had been exposed in 20 minutes after anæsthesia was begun.

After returning to bed pulse was 80 and general condition good; not much disturbance from the operation except at no time did the patient become clear in his mind, arousing only enough to bring his hand to the right side of the head when asked if there was any pain. There was considerable bleeding from the wound which made it necessary to change the dressing the next morning. The temperature began to rise shortly after the operation and increased steadily up to midnight when it was 104.5. The pulse remained at about 80, but the respiration became more rapid rising to 40 a minute and showing a Cheyne-Stokes character.

January 19th. The stupor deepened, temperature dropped some, pulse rose gradually from 82 at midnight to 140. The respiration was more regular and less frequent during the day. By night it was again Cheyne-Stokes, and had reached 46 a minute. The amount of kidney secretion was fair, the urine and feces passing involuntarily. The patient grew rapidly weaker. Temperature stayed about 104, respiration 48 and of same character, pulse weakened and death followed 24 hours after operation with the temperature 105.5 an hour previous. At no time after operation was there any evidence of a change in the left-sided paralysis.

Pathologic Findings. The brain was removed and hardened in formaldehyde. Beneath the skull, outside the dura there was a clot about $\frac{1}{4}$ inch thick, which had probably formed at the time of the marked hemorrhage a few hours after the operation. Aside from this nothing especial was remarked in the gross appearance except a very general atheroma of the vessels. The brain was not sectioned until it had been some days in the fluid.

The sections were made transversely, the slabs measuring $\frac{1}{2}$ inch in thickness. The right hemisphere was markedly enlarged throughout by a glioma which could be differentiated from the normal brain only by its whitish color. It occupied the major portion of the frontal lobe and much of the parietal. Beginning an inch from the frontal pole it

broadened as it went back. In places it approached the cortex to within a few m.m. The extent is shown in Figure I. In the center of tumor there were two necrotic areas, filled with bloody detritus. The smaller area nearer the median line was of a brighter red color and probably more recent. The necrotic area lay beneath the posterior and anterior central gyri and was very close to the surface. Figure II is a transverse section at the middle of the third ventricle and shows the general increase in size and the extent of the encroachment. The heavy shaded areas represent the necrotic hemorrhagic areas. Microscopically the tumor is a glioma.

Doctor N. M. Semple made the ocular examinations, with the following results: First examination November 29th, 1907. After correcting a myopia of 1 d., vision normal in both eyes, 20-15. Ophthalmoscope showed a deep physiological cupping of both discs. Central portion of



Fig. II. Transverse section at the center of third ventricle, showing increase in size of parietal lobe, and the extent of the glioma. Darker areas are degenerated with hemorrhages.

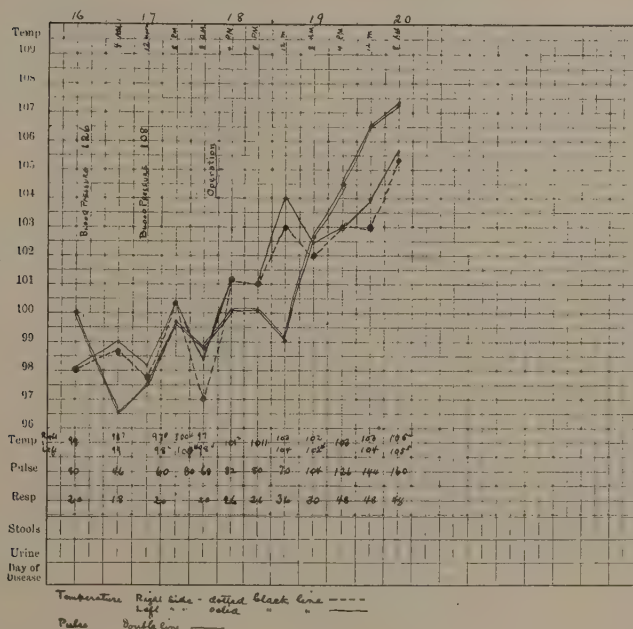
discs rather white. No hyperæmia, no oedema. Retinal arteries showed arterio-sclerosis, more marked in the lower temporal branch of right central artery. Examination of fields of vision showed decided concentric contraction of fields for red and green; red field of left eye more contracted than that of right; the contraction of fields for green same in both eyes. Fields for form, white and blue very slightly affected (practically normal).

Second Examination, January 18, 1908. Slight hyperæmia of both discs. Edges somewhat "washed," arteries small and thread-like, veins slightly tortuous. No appreciable swelling of discs. Left cornea less sensitive than that of right—apparently anesthetic.

Remarks: The first clinical evidence of this tumor of which we have any account, appeared at least six months before our first examination in November. During all of this time there were none of the general or cardinal symptoms of brain tumor, although it must in this time

have gained a considerable size. Eight weeks before the autopsy there were no objective sensory signs in the whole left side of the body. The position of the tumor, as revealed by the autopsy, makes it evident that there must have been extensive sensory disturbance of an objective kind before the patient's death. These facts throw some light on the rapid rate at which this tumor had grown, which must have been very rapid, even making allowance for the well-known fact that gliosis, less than any other brain neoplasm, compromises the functions of cells and fibres. At a recent meeting of the American Neurological Association,

Name T. H. (Brain Tumor.) Admitted Jan. 16 1908



Dr. E. E. Southard, of Boston, cited an instance of very extensive growth of cerebral gliosis in the space of six weeks.

This case well illustrates the fact that marked subjective sensory phenomena, *i. e.*, paresthesia of various descriptions, often appear in the absence of objective sensory signs. The subjective often antedate for a long (?) time the appearance of the objective signs in the same areas or territories. We have observed this fact, *i. e.*, the later appearance of the objective signs, in lesions of the brain, of the cord and of the peripheral nerves, and have referred to it in previous papers.

DERMATITIS FOLLOWING THE USE OF WALNUT JUICE
HAIR DYE.

By W. A. HARDAWAY, M. D., of Saint Louis.

For the past five or six years I have been meeting with cases of dermatitis of the face, neck and ears, due to the application of a hair dye made of walnut juice. Some of the attacks have been trivial, others have been severe and painful. In all the cases that I have encountered, the patients seemed to be unaware of the cause of the trouble, and kept on adding fuel to the flame by repeated applications of the offending agent.

In some instances the dye has been home-made, presumably a decoction of the walnut hulls; or, on the other hand, the article employed has been a preparation bought from the apothecary. The symptoms produced have been the same from both sources, so it is possible that the commercial dye really contains walnut juice.

As I have just said above, the amount of inflammatory reaction varies in different persons. Sometimes the attack is acute, involves the face, neck and ears, and presents such an amount of accompanying edema that the eyes are closed and the features made unrecognizable. The subjective symptoms consist mainly of burning and prickling sensations and a feeling of intolerable tension.

In some persons the condition is merely one of subacute inflammation, renewed from time to time by the reapplication of the dye, and limited to the ears and the skin at the margin of the scalp. Again, in persons of an "eczematous habit," to use an old expression, the dye has apparently evoked a veritable eczema, which has extended to the arms and trunk. The hairy scalp itself is not particularly involved, since, as is well-known, this region is naturally very insensitive.

It remains to be said that this untoward effect of the application of walnut juice is by no means universal, for this stain is in very common use as a hair dye, and only a relatively small number of people exhibit any idiosyncrasy in regard to it; still I believe that such accidents occur frequently enough to justify this brief note of warning.

BICHAT:
HIS LIFE, RESEARCHES AND CHARACTER.

By ALBERT C. EYCLESHYMER, B. S., Ph. D.
Professor of Anatomy, St. Louis University, St. Louis, Mo.

(CONTINUED FROM JULY.)

The first volume contains a detailed description of the bones of the skeleton, their articulations and movements, interspersed with chapters on development and the changes in old age. The second volume is devoted to a description of the muscles of the various regions, together with a discussion of the movements of each and the development of many. The anatomy of the larynx is considered in detail and the chapter closes with remarks on its development. The anatomy of the eye and ear are next considered, together with the development and old age changes. The first part of the third volume embodies a description of the anatomy of the central and peripheral portions of the nervous system. In the second part of this volume, Buisson discusses the alimentary tract. In the fourth, he describes the anatomy of the respiratory, vascular and lymphatic systems. The fifth volume by Roux is devoted to a description of the glands which are accessory to the alimentary tract, together with the peritoneum and the genito-urinary organs.

In this work, as in the general anatomy, he follows the principles laid down in his physiological researches. The organs are grouped in three great divisions. 1. Apparatus of animal life, under which he considered the organs of locomotion, the voice, external sensations coming through the eye, ear, nose, tongue, skin, and the internal, coming through the brain and spinal marrow. 2. Apparatus of organic life, under which he considered digestion, respiration, circulation, absorption and secretion. 3. Apparatus of generation, under which he discusses the anatomy of the generative organs and the foetal membranes.

In presenting a brief resumé of Bichat's Pathological Anatomy, the writer has first taken a few extracts from his general anatomy, in which he says: "It is not only the history of diseases that the anatomy of systems will elucidate, it will change in part the method of treating morbid anatomy. Morgagni, to whom we owe so much in this respect, and many others to whom the art is indebted, have adopted the general arrangement used in this description. They have examined the diseases of the head, chest, abdomen and extremities. In following this method they can only give us a general idea of the alterations common to all the textures."

"It appears to me infinitely more simple to consider at first all the

affections common to each system, and then to observe what every organ has peculiar to itself in the part that it occupies."

"It seems to me that we live at a period when morbid anatomy should take a higher stand. This science is not only that of derangements, that take place slowly as the principles or consequences of chronic diseases, but also it consists in the examination of all the alterations our organs can undergo at any period in which we may observe their diseases. With the exception of certain kinds of fevers and nervous affections, everything is within the province of this science. How weak appears the reasoning of many noted physicians, when we examine it, not in their works, but on the dead body. Medicine was for a long time excluded from the circle of the exact sciences; it will have a right to be associated with them, at least in the diagnostics of diseases, when we shall everywhere unite to accurate observation an examination of the changes the organs undergo. This course is beginning to be that of all rational minds; it will without doubt soon be general. What is observation worth if we are ignorant of the seat of the disease? You may take notes for twenty years at the bedside of the sick, upon the diseases of the heart, the lungs, the gastric viscera, etc., and all will be to you only a confusion of symptoms, which, not being united in one point, will necessarily present only a train of incoherent phenomena. Open a few bodies, this obscurity will soon disappear, which observation alone would never have been able to have dissipated."

Bichat's last course of lectures were on Pathological Anatomy, and although premature death has deprived us of any work from his hand which embodies his latest views, one of his zealous hearers gathered the outlines of his last course, and these came into the hands of Beclard and were found to be so complete that he transcribed them. At Beclard's death, they were published under the supervision of Boisseau. It is doubted by some if this work is to be relied upon. It was not published until three years after Bichat's death, yet the phraseology is so strikingly similar to that in Bichat's undoubted writings, that it seems more than probable that it is genuine. These lectures were grouped under twenty-three headings. In a preliminary discourse he briefly refers to the history of medicine, and points out that Morgagni truly created pathological science, and whilst yet in its infancy, he carried it to perfection, his work on chronic disease being a masterpiece. He says further, that "Anyone who has had the opportunity to see only a few post-mortem examinations, has observed that the condition of the organs, either internal or external, varies according to the affection which has produced death; we may distinguish three kinds of death: sudden death, that which happens in an acute malady and finally, that which follows a chronic disease." He next points out that the manner to proceed in autopsic examinations must differ according to the different kinds of disease we have to deal with; commonly it is prosecuted in an anatomical order, but it is not the best method to gain precise ideas on the subject; in gen-

eral, it is a great deal better to follow the order of the functions; in this manner we arrive at a knowledge of these which are injured." The remaining twenty-one lectures were as follows. 3. The alteration of fluids; first the circulation, then the exhaled and secreted. 4. On inflammation. 5. Diseases of the serous system, pleura, pericardium and peritoneum, the arachnoids, alimentary and genito-urinary systems. 7. Diseases of the cellular system, meaning all that tissue which fills up the interstices among the organ. 8. Pulmonary diseases. 9. Diseases of the glands. 10. Diseases of the cutaneous system. 11. Diseases of the muscles of organic life, heart, digestive muscular tissue, uterine tissue. 12. Diseases of the muscles of animal life. 13. Diseases of the arterial system. 14. Diseases of the venous system. 15. Diseases of the nervous system. 16. Diseases of the absorbent system. 17. Diseases of the fibrous system, including the dura matter, periosteum, aponeuroses, ligaments, tendons. 18. Diseases of the synovial system. 19. Diseases of the cartilaginous system; under which he remarks that we understand very imperfectly the diseases of the cartilages. 20. Diseases of the medullary system, spina ventosa, only remedy amputation, may occur at the extremity or in the middle of long bones. 21. Diseases of the osseous system, exostoses, fragilitas assium, and mollities ossium. 22. Diseases of the pilous system. The hairs enjoy so obscure a vitality that the greatest number of their alterations are not understood. There is only plica Polinica, which is susceptible of a particular description. It is not known in France, but is epidemic in Poland. According to the description given of the disease, it is hereditary and spontaneous, but not contagious, and is ushered in by several general symptoms. The hairs are matted and glued into inextricable tangles, they grow long and coarse and discharge an ichorous fluid from their extremities and bleed when they are cut." 23. Diseases of the epidermoid system. In all the above the pathology alone was considered, nothing of the therapeutics.

A sketch of Bichat's life would be incomplete which did not attempt to present his views on therapeutics. His later views the world can never know, but some information may be gathered from his words in the general anatomy. He says: "Examine all the physiological and all the pathological phenomena and you will see that there is no one which cannot be ultimately referred to some one of the properties of which I have have spoken. * * * * Every curative method should have for its object the restoration of the altered vital properties to their natural type. Every remedy, which in local inflammation does not diminish the augmented organic sensibility, which in oedema and dropsy does not increase this weakened property; and which does not reduce animal contractility in convulsion and elevate it in paralysis, fails in its object.

"To what errors have we not been led in the employment and denomination of medicines; we have created deobstruents when the theory of obstruction was in fashion, and incisives when that of the thickening of the humors prevailed. * * * * The same identical remedies have

been employed under different names, according to the manner in which they were supposed to act. Deobstruent is one case, relaxent in another, refrigerant in another, the same medicine has been employed with all these different and opposite views; so true is it that the mind of man gropes in the dark when it is guided only by vague opinion.

There has not been, in the *materia medica*, a general system, this science has been governed by the different theories which have successively predominated in medicine; each has, if I may so express it, flowed back upon the other. Hence the vagueness and uncertainty that it presents at this day. An incoherent assemblage of incoherent opinions. It is perhaps of all the physiological sciences, that which best shows the caprice of the human mind. What do I say? It is not a science for a methodical mind; it is shapeless assemblage of inaccurate ideas of observation, often puerile; of illusory methods and of formulae as fantastically conceived as they are tediously compounded. It has been said that the practice of medicine is disgusting. I add further that it is not, in some respects, the study of a reasonable man.

In the great hospitals at Lyons and Paris, he had an opportunity to follow the symptoms and manifestations of the many diseases. Here he could study the maladies of all countries and of all peoples. He studied experimentally the action of the various drugs in the wards of the hospitals. It is said that forty young men were selected to assist him in his work. He first studied the action of the drugs singly, and then in combination of two, three and so on, in order to ascertain what new therapeutic properties might thus be developed.

Early in July, 1802, he had been studying some macerated tissues which were in such an advanced stage of putrefaction that all his pupils had been driven away by the stench. On leaving the laboratory he had an attack of vertigo and fell on the stairs of the Hôtel Dieu, striking his head with such violence that he was unconscious for some time. Upon partially recovering from the injuries, he was attacked by what was then called putrid malignant fever, probably some form of septicaemia, and after fourteen days of illness he passed away on July 22nd, 1802, in the arms of his master's widow, whom he had continued to support and who repaid him by her loving care. All the professors of the faculty and upwards of six hundred students accompanied his body to the grave in Sainte Catherine.

Napoleon's famous physician, Corvisart, in writing the Emperor asking that a monument be erected in his memory said: "Bichat has just died upon a field of battle which numbers more than one victim. No one has ever done so many things in so short a time, nor so well."

Ten days after his death the following instructions were received by the Minister of the Interior:

"I pray you: Citizen Minister, to have placed in the Hôtel Dieu, a marble dedicated to the memory of citizens Desault and Bichat, which

shall attest the recognition of their contemporaries for the services which they have rendered, the one to French surgery, of which he is the restorer, the other to medicine, which he has enriched with numerous useful discoveries. Bichat would have greatly extended the domain of this science, so important and so dear to humanity, if pitiless death had not struck him down at thirty years."

I salute you,

"BONAPARTE."

On the 3rd of September, 1802, the Society of Emulation ordered a bust of its founder. In 1807, the same society ordered a medallion struck carrying the bust. In 1833, it placed a marble plate on the façade of the house where Bichat was born. In 1837, David (d'Angers), in making the frieze of the Pantheon, represents Bichat as dying with his head crowned with laurels. In one hand he holds his pen and in the other the manuscript of his book on Life and Death.

In 1830, the village of Lons-le-Saulnier erected a marble fountain, and upon this was placed a bust of Bichat by Huguenin. In 1843, the village of Bourge erected a marble statue of Bichat. This was also from the chisel of David. The great artist represents his hero in the attitude of work and meditation. His right hand rests upon the heart of a little girl who stands by his side. At his feet is an antique lamp, several dissecting instruments and a scroll, upon which one reads, "Researches on Life and Death."

In 1845, under the auspices of the Medical Congress, his remains were removed from the desecrated cemetery of Sainte Catherine to Père le Chaise. When the body was exhumed the skeleton was found in an admirable state of preservation, but the head was wanting. No little astonishment was created when Roux, his friend and physician at death, drew from his coat the skull of Bichat, which he declared he had possessed for upwards of forty years. When asked how he obtained the head he replied that it was useless to recall the circumstances. After placing a crown of laurels upon his head, his remains were again laid to rest.

In 1857, the statue which stands in the quadrangle of the school of medicine was unveiled with grand and solemn ceremonies.

In July, 1902, the French Society of the History of Medicine held memorial services on the centennial of his death. His tomb was visited at Père le Chaise where Blanchard gave an address pointing out especially the influence of Bichat on contemporary science. The house, at 14 rue Chanoiness, in which Desault and Bichat died, was then visited and Blanchard again made a few remarks on the intimate relation existing between Desault and Bichat; a commemorative plaque was then placed on the house. Later the members of the society and their invited guests assembled in the great amphitheatre when the beautiful tableau by Hersant representing the death of Bichat was taken from the cabinet and exposed for the admiration of friends. Addresses were then made by a number of eminent men and a medal was struck in memory of the occasion.

Having followed his brief career in running narrative and enumerated the honors which the world has conferred upon him, let us consider what his influence has been on the various branches of medical science.

Husson says that the anatomy was at this time bristling with details which repelled the young who were destined to study the art of healing. The prevailing conception was that each bone and organ conformed to a geometrical figure, and a fastidious nomenclature had grown up around this concept. His great service to gross anatomy was not in finding out new facts but rather in simplifying the nomenclature.

Bichat created general anatomy or histology. This work was the first in which the tissues were considered by themselves. Bichat states in his preface that the plan consists in considering separately and in detail each of the systems which through their diverse combinations form organs. Through the application of physics and chemistry to the study of the organs he was able to unveil many of the mysteries concerning their textures.

Gley says: "Bichat's description of the cellular serous and lymphatic systems and their relations is so precise that the later histologists only appreciated their worth little by little, although they were aided in their researches by powerful microscopes."

Serres writes that the secret of his great work was the principal of analogies. "It is this which dominates his histology, analogy of structures and analogy of properties give rise to analogies of functions and maladies. It is, I repeat the source of the imperishable work of Bichat and the reason of its useful application to the medical sciences."

Von Koelliker, the greatest histologist of the 19th century, says of Bichat's work. "Bichat's *Anatomie Générale* was the first attempt to treat histology scientifically and on this account merely, it constitutes an epoch; but besides this its importance was still greater inasmuch as the tissues were not merely clearly defined and fully and logically treated of, but full account was taken of their physiology and morbid alterations."

The founder of histology he was also one of the founders of modern physiology for he not only made a considerable number of important discoveries but also strongly advocated animal experimentation. This work had been carried on by Spallanzani, Haller and others; yet physiology was in many places still a subdivision of metaphysics. The great Larrey said: "He was already the greatest physiologist of his century and he must have been the greatest physician had he but lived twenty years more."

Concerning his influence on Pathology Cérèse says the "Pathological Anatomy which was but a collection of isolated facts Bichat raised to the rank of a science."

He clearly saw that human anatomy should be elucidated by comparative anatomy, and human physiology by comparative physiology. Anatomy and physiology should be correlated instead of physiology being a subdivision of metaphysics. Pathology should be based upon

anatomy and physiology in diagnosis, and the findings confirmed by the autopsy. Therapeutics instead of being abandoned to medical tact should be studied in the wards of the hospital. He conceived the grand idea of erecting a complete system of medicine based directly upon observed facts and at the time of his death he was busy on the gigantic task of classifying all diseases.

His style of writing was exceedingly loose, owing to the fact that he never revised his manuscript. His writing was very fine and without punctuation. He wrote on all sorts of paper, of different sizes and colors, margins of newspapers, old envelopes, etc.

Blanchard tells us that while Bichat was writing his general anatomy the editor employed a boy named Chaudé as a messenger to obtain the copy leaf by leaf as Bichat wrote. Hé wrote only at night and never copied that which was to be printed the following day. He was obliged to work without relaxation in order to furnish copy for the printer. His room was small, filled with books, bones and dissections. This human debris together with the remains of his meals which were brought to him vitiated the air with a sickening combination of odors. To this chamber of horrors Chaudé came early each morning for his manuscript and at times could scarcely awaken Bichat. Chaudé says that Bichat while dressing would often recite and act with great precision and in a most dramatic way the parts of the principal actors whom he had seen at the Palais Royale or Français.

His method in scientific work was to redemonstrate the known facts; to repeat the observations and experiments which had led to doubtful results. In the practice of medicine he applied the same principle of looking directly at nature with his own eyes. He was always on the alert for new facts and when these were established they were co-ordinated with other known facts. In the introduction to his descriptive anatomy he says: "Dissect in anatomy; experiment in physiology; follow the disease and make the autopsy in medicine. This is the threefold path; outside of which there can be no anatomist, no physiologist nor physician. Look at all the great men whose names are inscribed in the annals of these sciences they have constantly followed this path. If I have attained any success it is because I have studied nature more on the cadaver than in anatomical works, more in the living animals than in physiological writings, and because I have studied more at the bedside than in medical books."

The celebrated Sandifort of Leyden wrote to a friend saying that "in six years your Bichat would have passed our Boerhaave." Tillaux says that "up to the time of Pasteur no one had had such grand and fertile ideas in medicine." Cérésé states that "medical genius has never at a single bound raised itself to so great a height."

Not only did Bichat create a new epoch in medical science but also as Blanchard has stated, he had a considerable influence upon modern philosophy. Schopenhauer would permit his students to discuss

philosophy and psychology only after they had read Cabanis and Bichat while Auguste Comte, the creator of positive philosophy attributes to Bichat a preponderant influence upon the progress of science in general.

From Buisson's life of Bichat we learn much of his personality. He was a man of prepossessing appearance and amiable disposition, never given to anger or impatience. Although a man of mirth he never descended to be a merry-andrew. His generosity towards students both in giving time and money was a resource to the backward and needy. Quick to recognize talent he adopted every means of developing it, yet he never employed those ornate laudations which so often accompany the insincere compliment. He was always as accessible when engaged in the most laborious work as in moments of leisure. It was no trouble for him to come down from the position of schoolmaster to that of schoolmate.

He never hinted that he had walked among the learned, his every act was a proof that needed no further elucidation. A man of great frankness and candor yet always ready to sacrifice his opinion when a well founded objection was offered. In clearing the field of science of dogmas and delusions he did not engage in wordy warfare nor resort to bombastic rhetoric well realizing that many a noisy reputation fails to awaken an echo in posterity, instead he diligently worked away cutting off this root then that until no source of nourishment remained.

All the great minds loved him, yet there were little minds who sought to rob him of his hard earned laurels, not being able to pardon his ability. He was content to suffer their attacks; he never gave a direct rebuff, but always sought to restore the good fellowship which his malevolents themselves had broken. He had a soul ever warm, showing tenderness towards the hard, patience with the impatient, generosity to the selfish, and philanthropy toward the misanthropic. He dwelt in a perpetual Utopia of his own in which he never ceased to read the infinite book of nature of which the wisest know little more than the introduction. His philosophy springs neither from the classic hall nor scientific laboratory but from a magnificent human soul.

Like his noble father, his saintly uncle and his great master Desault, he gave to mankind his wealth, knowledge and life.

When we realize what he did in that brief life of thirty years we are not surprised that those who watched his career stood by with bated breath. Nor do we wonder that he who to-day reads the marvelous story feels the heart beat faster and the soul thrill with emotion as he exclaims in admiration, "Oh, wonderful Bichat."

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MEDICAL AND SURGICAL PROGRESS.

THE DETERMINATION OF TRACES OF BLOOD (OCCULT HEMORRHAGE) IN THE FECES AND STOMACH CONTENTS.

A REVIEW OF RECENT LITERATURE.

By JESSE S. MYER, M. D.

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In 1893, H. Weber presented his observations and results in examinations of the feces and stomach contents for traces of blood. This essay was the basis for future work along this line and, with but few minor improvements in technique, the test described by him at that time remains today one of the very best at our disposal. He recognized, as had those working before him, the difficulties of demonstrating small quantities of blood in the feces and stomach contents and yet appreciated the great importance of a test that would enable one to demonstrate its presence in cases of "suspected ulcer of the stomach and duodenum and carcinoma of the intestinal tract." He realized, too, in view of the fact that blood was given off only from time to time from the surfaces of these lesions, the necessity of making repeated examinations of the excreta. At that time the clinical diagnosis of blood depended upon (1) the microscopic demonstration of red blood corpuscles; (2) the test of van Deen which depended upon the principle that the brown solution of guaiac is colored intense blue by blood upon the addition of

turpentine; (3) Teichman's test, the micro-chemic demonstration of haemin crystals; (4) Heller's test, upon the addition of potassium hydrate to urine and boiling there is a red color of the precipitated phosphates due to blood pigment; (5) the spectralanalysis.

All of these tests had been proven inadequate in the detection of traces of blood in the feces and stomach contents. The microscopic examination was unreliable because in both the stomach and intestines the corpuscles are quickly destroyed. It is noteworthy here that Nothnagel called attention to the fact that even in cases of considerable hemorrhage in typhoid fever he had not been able to demonstrate the corpuscles in the feces.

Van Deen's test, upon which Weber based his investigation, was unreliable because the same reaction was given by many vegetable rests, and many inorganic substances. This rendered it impracticable in the examination of the feces in which there occur both vegetable rests and inorganic substances. The Teichman test requires that the blood be in a rather concentrated form and is very satisfactory where particles, or drops, may be selected, but when a small quantity of pigment is diffused through a large mass of feces, the test is not sufficiently delicate. For this reason, Vierort recommended washing the blood out of the stool, evaporating the water, and testing the residue. The Heller test is not even exact in the examination of urine, much less feces, where other pigments are present.

The spectroscopic examination while reliable, presupposes the possession of a good spectroscope and experience in the use thereof. One inexperienced might easily, for instance, confound the chlorophyll line in the red with that produced by blood pigment. It is necessary to obtain the blood pigment in a clear solution, in a fairly concentrated form, and free from all other coloring matter which might give a similar spectroscopic scale.

Seiffert and Mueller recommended in this connection that the feces to be examined be first mixed with water to which a few drops of concentrated acetic acid had been added and then extracted with 1-5 of the volume of ether. The ether rises to the top stained with the coloring matter. If it remains foamy or opaque, a few drops of alcohol may be added to clarify it. The process of digestion converts hemoglobin into hematin, which is taken up by the acetic acid-ether, the ether may then be examined in the spectroscope and gives a definite absorption band in the red which needs to be differentiated from chlorophyll only. Weber, prompted by the desire to simplify a test which might be of general clinical value, ingeniously combined the step recommended by Seiffert and Mueller with the old van Deen test with guaiac and turpentine and published to the world the test which has since borne his name. In a few words his original test is as follows:

"A portion of feces is mixed with water to which has been added 1-3 of the volume of concentrated acetic acid. It is then extracted with ether. Of the acetic acid-ether extract several c.cm. are poured off to which is added 10 drops of guaiac tincture and 20 to 30 drops of turpentine. In the presence of blood the mixture becomes a blue violet, in its absence a reddish brown.

The old van Deen test gave positive reaction according to various authorities not only to blood but to (1) certain food stuffs, vegetable rests, and milk; (2) products of the body, bile, saliva, pus, etc., and (3) many inorganic substances which are used internally as medicaments.

In a long series of examinations with foods of various kinds, bile, pus, and sixty-seven different drugs, the author found that after ex-

traction with acetic acid-ether the application of the van Deen reagents gave no positive reaction except in the case of raw or partially cooked meats. He recommended, therefore, that these be withheld from the diet of those being examined. He saw no objection, however, to well cooked meats. He found that 3 c.c.m. of blood ingested was sufficient to give a decided positive reaction in the feces.

The results of Weber's observations, over fifteen years ago, have been improved upon but little. The subject received comparatively little attention from clinicians for a period of 7 years and did not receive its merited clinical significance until Boas in 1901 published the results of his observations.

The above articles present the mature observations and results of Boas and are, perhaps, all in all two of the most satisfactory publications on the subject. It was he who applied the term "occult hemorrhage" and defined the same to be a hemorrhage so slight as not to manifest itself in the feces or stomach contents by a change of color. In other words, for a hemorrhage to be occult, it must not manifest itself macroscopically or microscopically. The accidental presence of blood as from nosebleed, hemoptysis, hemorrhage from the teeth and gums, slight hemorrhages due to injuries of the mucous membrane by the introduction of the stomach tube, etc., etc., are also excluded under the term "occult hemorrhage." He recommends highly Weber's test with slight modifications, such as the use of old ozonized turpentine, as recommended by Schumm, and the addition of powdered guaiac directly to the ether, because the tincture does not keep well. Instead of the old turpentine, which cannot always be had in the proper state, hydrogen peroxide may be used, 10 to 15 drops of a 3 to 5 per cent. solution.

Boas finds Rossel's modification a reliable and useful one. It consists in the use of aloin (barbaloin) instead of guaiac, the other steps in the test remaining the same, the end reaction being a cherry red color. The affections of the gastro-intestinal tract in which blood may be found are ulcer of the oesophagus, carcinoma of the oesophagus, ulcer of the stomach in which the blood may be transitory, depending upon the condition and irritation of the ulcer, and carcinoma, in which blood is permanent in the feces regardless of the mode of living. In the intestines, ulcer of the duodenum, typhoid ulcers, tubercular ulcers, carcinoma of the intestines, etc., Boas calls special attention to the interesting fact that in both ulcer and carcinoma of the stomach, blood may be found in the feces and yet not be found in the stomach contents. This he attributes to the vertical position of the stomach and the trickling of the blood into the duodenum along the walls of the stomach without being admixed with the contents. In the observation of 257 cases, Boas and Kochman obtained negative results in chronic gastritis, atony, hypersecretion, achylia gastrica, hyperacidity and neuroses. They found the results variable in ulcer of the stomach and duodenum for reasons mentioned above. It was constantly positive in carcinoma of stomach and intestines, being found sixty-five times in sixty-seven cases.

Clemm gives a most careful review of the whole subject both with reference to technic and clinical significance. He describes in detail the acetone-haemin test of Nencki, though still preferring the guaiac turpentine test. He even suggests that it may be possible later to differentiate the blood of cancer from that of other conditions through crystallography.

Schumm, in 1906, presents an exhaustive study of the subject and recommends, after a careful review of the various tests, that of Weber with certain modifications. He first extracts from the feces the neutral fats, free fatty acid, and coloring matter with alcohol and then treating

the feces thoroughly with glacial acetic acid destroying the oxidation ferments. A portion of the ether then obtained is tested with guaiac turpentine and another portion with the spectroscope.

In 1904 O. and R. Adler described the benzidin test maintaining that it was vastly more delicate than that of Weber. 3 c.cm. of a saturated alcoholic solution is mixed with 2 c.cm. of a 3 per cent. hydrogen peroxide solution, to which are added a few drops of acetic acid and a small quantity of feces in water suspension. In the presence of blood, an intense green is the result. They found that oxydizing ferments, both animal and vegetable, give the same reaction and found the reaction positive to blood in a dilution of 1 to 100,000. On account of its great delicacy, which is not altogether desirable, and the fact that various other substances in the feces give a like reaction, the test did not meet with the support that time has shown that it merited.

Schlesinger and Holst, working in Strauss' laboratory, in 1906, succeeded in overcoming to a large extent the chief objections to the Benzidin test, through the recommendation that boiling the feces destroys the oxidizing ferments and that by taking a small definite amount of the feces the sensitiveness of the test is diminished. They recommend the following: As much Benzidin as can be held on a knife point is added to 2 c.cm. of acetic acid. To this is added 2 to 3 c.cm. of a 3 per cent. hydrogen peroxide solution. A portion of the feces as large as a pea is boiled in 1-5 of a test tube of water. Of this a few drops is added to the above reagent. A beautiful blue, or in the presence of very small amount of blood, a greenish color is the end result. The test with these modifications has become very popular and at present is pretty generally conceded to be quite reliable. Schumm, while commending the test on the whole, still prefers his modification of Weber. His objections, however, do not seem well founded and are certainly not vital, as has been shown by Schlesinger and Holst in their answer to his objections. Isler sums up completely the entire literature in a recent publication which is worthy of the perusal of any one desiring a comprehensive knowledge of the subject.

The following are a few precautions that are recommended in the application of the tests for occult hemorrhage in the stomach contents and feces:

The elimination from the diet of meats, cooked and uncooked, and meat broths for a period of three days prior to making the test; the elimination from the diet of substances containing iron, copper, iodides, bromides, etc., the use of ozonized turpentine since fresh turpentine seems to give varied results; the boiling of the feces in the Benzidin test to destroy oxidation ferments; the extraction of fats from the feces before applying the tests, and the neutralization of hydrochloric acid in the stomach contents because it interferes with the haematin reactions; a thorough cleansing of test tubes because of the possibility of contamination, for instance, with Fehling's and Trommer's reagents, etc.; the fingers should not be used as stoppers in shaking the test tubes because of sweat which may contaminate the reagents; great care must be observed in the use of the stomach tube in obtaining stomach contents because of injuries that may be produced to the mucous membrane, and thus an accidental contamination with blood; the possibility of accidental hemorrhage from the mouth and upper respiratory tract and the rectum and anus must be eliminated; it is well to see that the bowels are moving freely prior to making the test because an impaction of feces may cause slight injuries to, and hemorrhages from the mucous membrane.

THE SIGNIFICANCE OF NELATON'S LINE AND THE DIFFERENCE IN LENGTH IN THE LOWER LIMBS.

A REVIEW OF RECENT LITERATURE.

By NATHANIEL ALLISON, M. D.

1. THE ROSER-NELATON LINE AND THE CAUSES AND SIGNIFICANCE OF ELEVATION OF THE TROCHANTER.—Preiser (*Zeit. f. Orth. Chir.*, Bd. XIX, Hft. 1-2).
2. THE RELATION BETWEEN THE TROCHANTER AND ROSER-NELATON'S LINE.—Saxl (*Zeit. f. Orth. Chir.*, Bd. XVII).
3. THE ROSER-NELATON LINE.—Vogt (*Zeit. f. Orth. Chir.*, Bd. XVI, Hft. 3-4).
4. HEINE'S OPERATION OF CONTINUITY SHORTENING TO COMPENSATE FOR DIFFERENCE IN LENGTH IN THE LOWER LIMBS.—Deutschlander (*Zeit. f. Orth. Chir.*, Bd. XIX, Hft. 1-2).
5. CONTINUITY-RESECTION OF LONG BONES TO COMPENSATE FOR SHORTENING.—Glaessner (*Zeit. f. Orth. Chir.*, Bd. XIX, Hft. 1-2).
6. INCREASE IN LENGTH OF THE WHOLE FEMUR FOLLOWING WHITE SWELLING OF THE KNEE.—Mayet and Bourganel (*Arch. d. Med. des Enfants*, Tome X, No. 5).
7. CONGENITAL BENDS AND PSEUDOARTHROSES OF THE LEG.—Gasne (*Revue d'Orthopedie*, May and July, 1907).

Preiser has made observations on one hundred living normal persons and has shown that Nelaton's line and the elevation of the trochanter of the femur have not the diagnostic value in determining pathologic conditions at the hip joint that has been generally conceded. In his series of cases he found the trochanter above the line in one-half. He also examined 103 specimens in a museum, placing a stretched string between the anterior superior spine of the ilium and the tuberosity of the ischium and observing its relationship to the acetabulum. In 43 of these cases the relations were what is regarded as normal; the remaining 60 cases he divides into two groups:

In the first group the acetabulum was placed in the frontal plane, was very shallow and caused the trochanter to be above the line, due to the fact that the femur rotated backwards in order that the foot might be placed in the sagittal plane. This group contained 29 per cent. The second group, made up of 28 per cent., revealed an acetabulum placed dorsally, resulting also in elevation of the trochanter above the line. The author concludes that elevation of the trochanter may take place without a change in the angulation of the neck and shaft of the femur, and may be due to either dorsal displacement of the acetabulum, mesial displacement of the acetabulum, or a shortened femoral neck. Saxl has also observed that there are cases where the angle and neck of the femur are normal in which the trochanter is above the Nelaton line, and that this may be due to the fact that the neck of the femur is short, or that the trochanter is over-developed, or that these two factors may occur together. To offset the danger of error in making nice measurements at the hip, he suggests that the thigh be flexed to a right angle, that it be neither abducted nor adducted, neither inwardly nor outwardly rotated. This position brings the upper edge of the trochanter under normal conditions on the line. Voigt has investigated the value of the Nelaton line on the cadaver by an exact system of measurements, and has found that in full extension of the leg where there is neither inward nor outward rotation, that the trochanter falls slightly below

the line. In order to cause it to fall on the line abduction is necessary, the amount of which varies in different individuals. If the thigh is flexed, however, to an angle of 135° without abduction or adduction, the trochanter will fall 0.5-1 cm. above the line, adduction being necessary to cause it to fall on the line; the amount varying also with the subject. He suggests the value of a method of measurement used by Lange, wherein the crest of the ilia and the trochanters are marked out on both sides with a colored pencil. The patient then stands or lies face downward. If there is difference between the two sides, it can be noticed at a glance and comparative measurements can be easily made.

Heine devised an operation for obtaining shortening in continuity to compensate for difference in the length of the lower limbs. Deutschlander has reported three cases in which this operation was done. The operation necessitates careful measurement in order to ascertain the amount of shortening to be required. When this is determined, an exact amount of bone is removed and a small aluminum splint is placed between the fragments, being held in place by two screws. This is removed at the end of four weeks, and the wound allowed to heal. The three cases reported were good results. Glaessner also reports three cases where an operation was done to compensate for shortening. One had shortening due to congenital dislocation of the hip, the second to a badly united fracture of the femur, and the third due to an operation. An oblique osteotomy was done on the femur on the healthy side of these cases, and the bone fragments were allowed to over-ride until the leg was of equal length with its fellow. Glaessner is of the opinion that each of these cases presents an individual problem which must be solved according to the mechanical conditions to be met.

Mayet and Bourganel have reported an interesting case which sheds some light on the pathology of disturbance of growth causing inequality in the lower limbs. The patient was 7 years old; had tuberculosis of the knee, which was treated for two years by fixation and plaster; at the end of 3 years more, during which time there was no treatment, the limb was discovered to present muscular atrophy and a lengthening of the femur of 4 cm., the tibiae being equal in length. Careful measurements on radiograms revealed an increase of 1 cm. in the height of the epiphysis at the knee and a lengthening of the adjoining portions of the diaphysis equal to about $\frac{1}{2}$ cm., while the neck and shaft of the femur formed almost a straight line. This accounts for the lengthening, and demonstrates the fact that developmental abnormalities of the epiphysis adjoining an affected joint does not entirely explain the lengthening in these cases, but that the absence of pressure due to immobilization and non-weight-bearing, resulting in an increase in the angle at the neck of the femur, plays a very significant part.

Gasne has pointed out the importance of congenital bends and pseudoarthroses of the leg as a cause in inequality and deformities of the lower extremities. He believes that arrested development is an explanation of these conditions, and that they are not due to intrauterine fracture. Congenital bends of the tibia most often occur at the junction at the middle and lower thirds without abnormal mobility. Congenital pseudoarthroses may not be evident at birth but may appear several years later after slight trauma. Congenital bends should be treated by protective apparatus, as forcible correction is apt to be followed by pseudoarthrosis. In severe cases, apparatus applied for weight-bearing is often not well borne by the poorly nourished limb, and a plastic bone operation or amputation is necessary. Pseudoarthroses demands apparatus, protecting or stimulating measures, or amputation. Where the bones are fairly solid, bone operation offers a fair chance of firm union.

THE PARATHYROID GLANDS.

A REVIEW OF RECENT LITERATURE.

By CARL FISCH, M. D.

1. Thomson (*Amer. Jl. of Med. Sciences*, Vol. 134, 1902).
2. Forsyth (*British Med. Journal*, May 18, 1902).
3. Vincent (*Lancet*, August 16, 1906).
4. Richard (*Virch. Arch.*, Band 191, Helt 1).
5. Geztown (*Virch. Arch.*, Band 188, 1907).
6. Erdheim (*Grenzgebiete der Medizin u. Chirurgie*, Vol. 16, p. 633).
7. Ungerman (*Virch. Arch.*, Vol. 187, 1907).

The interest in the function of the parathyroid glands and of most of the other so-called glands with internal secretions, has lately increased greatly, and during the last three years the number of published articles upon this subject has been so great that a general review of them all would fill a large volume. In the following remarks, therefore, only the publications that bear upon the morphologic and the histogenic characters of these formations or organs can be dealt with, leaving aside; almost altogether, the investigations into their functional quality and the efforts made to prove its existence by experimental work, leading finally to the production of specific sera. This phase of the subject may be discussed in a future number of the JOURNAL.

Present interest lies in the discussion about the specificity of the structure and position of the two organs, the thyroid and the parathyroid, histogenetical or developmental and functional. Opinions differ in regard to the effect of loss of these organs and it is only the biologic study of the character of the structures which makes it probable that we must adhere to the theory of a specific difference between them in both instances. As we have already said, the biologic investigation has made this almost positive, although we must not forget that biologic research along these lines is surrounded with so many obstacles and obscure phenomena that we can not regard as incontrovertible any conclusions arrived at as a result of such investigations. As an instance of the instability of such conclusions, we may refer to the hypothesis that, among others, the picture of a paralysis agitans is produced by certain changes in the structural character of the parathyroid glands. Thomson's investigations have shown us that such a relation does not exist. It is usually the structure and changes of tissue of these structures that occupies us in the discussion of work in this direction, and here the anatomic as well as the histologic qualities are to be considered. The question whether parathyroid and thyroid are developmentally and histologically different tissue complexes was not raised until the work of Forsyth and Vincent became known. Both these authors insist upon the correctness of their interpretation of their findings in human and animal tissue, that transitions in the tissue qualities of these two organs can be demonstrated. In view of the fact that the developmental origin of both is as yet not definitely settled, whether both arise from the tissue of the same bronchial cleft or from two different ones, such an assertion can not be accepted as the expression of real processes, because anatomic relations of the parathyroid to the thyroid vary greatly. While the ordinary

location of the first is a definite one in many instances, there are so many observations of a different location of one or the other of them that a regularity in this respect can not be considered. On the other hand, it is very often found that a whole and perfectly normal and definite parathyroid is imbedded in the substance of the thyroid, and from this finding to small portions of parathyroid tissue from small nests to more massive quantities of parathyroid tissue in thyroid tissue have been observed very frequently. It must be remembered that in the region of these organs, as is well known, many displacements of other structures also occur, for instance the finding of typical thymus foci in the thyroid and parathyroid. As most of these nests of parathyroid within the thyroid are definitely defined from the latter, very often surrounded by a distinct connective tissue capsule, a transition between the one and the other is never observed. The latest and most conclusive study of this occurrence was made by Richard, who conclusively demonstrated the independence of those misplaced areas of parathyroid tissue from the tissues of the thyroid. Geztown maintained that the epithelial cells of the parathyroid are of four different types, while Forsyth demonstrated that the different appearances and character of these cells were only the expressions of the stage of activity or rest of these glandular elements, the same as is observed in other glandular structures, for instance the pancreas. The same author, however, has found that the character of the secretory products can not be distinguished, either physically or microchemically, from that of the thyroid. He has not, however, investigated the presence or absence of iodothyroin in the parathyroid, the presence of colloid alone is of no meaning. It must be said also that no other observers besides Forsyth have ever found colloid in a parathyroid tissue.

The histology of the parathyroid has been, until recent years, clearly established. Through the study of Thomson on a great number of these organs a clear understanding of this question has been reached, and differentiates the two organs definitely. With this in view the suggestions of Forsyth ought to be critically investigated.

The pathologic changes in the parathyroid that have been described, consist in alteration of the epithelia,—as degeneration or atrophy,—in changes of the location of the glands, their variability in number and the inflammatory processes are also found, not primary infections but always in connection with a process elsewhere in the body. For the thyroid the same obtains.

The relation of such pathologic alterations to certain diseases has been made the object of a great deal of observation and experimentation. For the latter the conclusions have been to a high degree, in many cases, contradictory. This may be explained by the well-known uncertainty of the location of the glands, their variability in number and the rather frequent occurrence of misplaced parathyroid tissue into the thyroid. So even the removal of all of the four glands does not always give the certainty that all of the specific tissue has been eliminated; a small focus existing somewhere in the neighborhood will necessarily vitiate the conclusivity of the experiment. Many, for instance Forsyth himself, have abandoned rabbits on account of the great variation in number and location of the glands in these animals. Erdheim, for this reason, selected white rats for his work, as they have only two parathyroids and these almost always in the same place. The presence of accessory parathyroid tissue is in these animals easily excluded by making serial sections through their neck, that in these animals is very thin. The

consequence of these difficulties is that for no diseases circumstantially related to the parathyroid function, has there been established thus far any conclusive evidence for this etiology. Even for tetany, in children and in grown persons, the results of the study of the glands are not uniform, so that some writers have been forced to assume only functional alterations that do not express themselves by histologic changes. Although the coincidence of lesions of the parathyroid and tetany is certainly very convincing, we cannot yet conclude that every case of tetany is based on this causation. That in rabbits and other animals, by ablation of the thyroids, tetany can be produced, and that the introduction of extracts of the glands of parathyroid, proves that animals may be prevented from acquiring tetany, is true. But whether this is really the cause or only a coincidence can not be decided at present although the probability of its correctness is very great. Chronic lesions of these organs are certainly frequently found after death without any signs of tetany or other disturbance during life. On the other hand cases have been reported but they were cases with absent or very small thyroids, showing no symptoms either of myxedema or tetany, and on autopsy the explanation was found in a case lately reported by Myermann, as a tumor of the size of a chestnut at the root of the trunk. The parathyroids were normal. This is to be considered, according to the author, as evidence for the difference of function of the thyroid and parathyroid. Thomson reports 12 cases of infantile atrophy, that were mentioned before, finding changes in the parathyroid mainly fibrous. He discovered similar conditions in other ductless glands of the same cases, but is careful not to connect directly these findings with the general atrophic condition of the infants.

The subject of specific treatment by serum is still in its infancy. It promises, however, to elucidate in the future the question whether alterations of function or structural changes of the parathyroid glands are really at the bottom of diseases that indirectly suggest this origin.

DIAGNOSTIC AND THERAPEUTIC NOTES.

TREATMENT OF SCABIES.—Franz Nagelschmidt (*Med. Klinik*, 1907, No. 35).—To procure a rapid and certain result in the treatment of scabies, it is necessary that the remedy used does not irritate the skin; yet it must penetrate rapidly and kill not only the acarus, but also the ova. A new remedy, fulfilling these requirements, is thiophinol; it is used in the bath and as a 5 or 10 per cent. ointment. The technique is as follows: The patient takes a thiophinol bath of 30 minutes' duration, and immediately after this he is carefully rubbed with 30 to 40 grams of a 10 per cent. thiophinol ointment. The same ointment is applied once daily for the next two or three days, and on the fourth or fifth day a bath is taken, as in the beginning. In this way a cure may be accomplished in five days, and a secondary dermatitis, due to the remedy, is never seen. Thiophinol has the advantage over other sulphur preparations in that it penetrates the skin more easily and is far better resorbed. Healthy individuals with an intact skin, when taking a thiophinol bath, show a large amount of eliminated sulphur after one day.

THE PROGNOSTIC VALUE OF BLOOD PRESSURE IN PULMONARY TUBERCULOSIS.—A. B. Marfan (*Rev. de médecine*, November, 1907).—In pulmonary tuberculosis the blood pressure is usually low. In those cases, however, in which it is high, we can make, according to Marfan's investigations, a good prognosis. Such a finding means that the tuberculous process is on the high-road to a clinical cure. He has found hardly any exceptions to this rule and believes that the behavior of the blood-pressure gives us our most trustworthy prognostic information in pulmonary tuberculosis. A very low blood-pressure is of ill omen and indicates a rapidly progressive infection. This rule has however more numerous exceptions than its converse; a low blood-pressure does not always exclude the possibility of a cure. In his blood-pressure work, Marfan used the Potain sphygmomanometer applied to the radial artery with the patient in the seated posture.

TREATMENT OF MORBUS BASEDOWII.—C. Mes (*Nederlandsch Tydschrift voor Geneeskunde*, 1908, I., No. 16) recommends goat's serum in morbus Basedowii. He reports the case of a female patient suffering from Graves' disease, in which all the cardinal symptoms were present; *i. e.*, exophthalmus, goitre, tremor, tachycardia, dilatation of the heart, irregular pulse (so frequent that it was hard to count), lack of appetite, diarrhea and loss of weight. Bromides, iodides, quinine, iron and strophanthus, had no effect whatever. The writer removed from a goat the glandula thyreoidea and had the patient drink the milk of this goat for six weeks, while for three weeks afterwards 5 cc. of the serum of the goat were taken daily in a glass of Malaga wine. Very little improvement took place, which suggested that the glandula thyreoidea had not been entirely removed. The same operation was therefore done in a second goat and care was taken that this time the entire gland was removed. Patient again took the milk and 5 cc. of the serum daily at the same time. Patient improved slowly, but constantly. All the symptoms of the disease disappeared gradually and the patient is now entirely free from any symptoms; her weight, which was 53 kg. at the height of the disease, increased to 85 kg.

DIFFERENTIAL DIAGNOSIS BETWEEN EXUDATE AND TRANSUDATE.—W. Janawski (*Berl. klin. Wochenschr.*, No. 44, 1907).—An easy method to differentiate between these two is the following: A few drops of the fluid to be examined are dropped into a solution of 2 drops of glacial acetic acid in 100 cc. of water. If an exudate, a white or whitish-blue streak is produced, sinking gradually to the bottom and forming a whitish precipitate. If a transudate, nothing is seen, the drops dissolving completely before reaching even the middle of the solution.

TELERÖNTGENOLOGY.—Alban Köhler (*Deutsche med. Wochenschrift*, 1908, No. 5) describes a new method for examination of the location and dimensions of the heart by means of the Röntgen ray. The x ray tube is placed at a distance of 1.5 to 2 meters from the thorax, so that the rays go through the thorax practically parallel. It is possible to photograph the heart in its true dimensions by this procedure. The method, simpler than the orthodiagraphy, has the advantage that not only some points of the heart but the entire circumference of the heart appears on the plate.

LATENT CANCER OF THE CECUM. Lereboullet and Tixier (*Société Médicale des Hopitaux*, March 13, 1908).—At a recent meeting of the Société Médicale des Hopitaux of Paris, Lereboullet and Tixier report an instructive case of a man in whom, for over a year, the only symptom of a cancer of the cecum was apparently a marked anemia. Although towards the end of the patient's life some intestinal disturbances were added to the anemia, it was only the autopsy that revealed the presence of a very small and non-adherent cancer at the cecum. The anemia, moreover, was not at all of the type characteristic of cancer, being marked by a leucopenia and the presence of numerous nucleated red cells. As the writers point out, this case shows that just as we have gastric cancer revealing its presence only through an anemia, so we may have analogous cases of cecal cancer without other symptoms. Moreover it shows that the distinction between a carcinomatous and a primary anemia is by no means always so sharp as we like to assume. In the discussion, Gailhard reported a case of gastric cancer without any digestive disturbance and characterized only by a marked anemia.

THE MODE OF PRODUCTION OF THE PRESYSTOLIC MURMUR IN MITRAL STENOSIS.—Colbeck (*The Practitioner*, February, 1908).—Many different theories have been advanced to explain the occurrence of the presystolic murmur in mitral stenosis but none of them seems quite satisfactory. Colbeck believes, on the basis of extensive anatomical and clinical work, that this murmur is due to vibrations set up in the large anterior flap of the mitral valve during auricular systole. The short, sharp first sound heard in mitral stenosis must be due to the abnormally sudden and forcible closure of the mitral valve, which is occasioned by the ventricular systole. The closure of this valve is more violent than usual, for the reason that the large anterior curtain exposed to the impact of the inflowing blood stream is not floated up so near to the mitral opening during the filling of the ventricle as under normal conditions, and with the occurrence of the ventricular systole the vibrating flap is suddenly and forcibly impelled into the position which it occupies during the closure of the valve. This explains the sudden cessation of the murmur and the occurrence of the short, sharp first sound. When the anterior flap of the mitral valve is destroyed or is so tied down as to be unable to vibrate, we have the presystolic murmur disappearing entirely, an observation often made in mitral stenosis.

CORRESPONDENCE.

LONDON LETTER.

[FROM OUR OWN CORRESPONDENT.]

It is most sincerely to be hoped that the wish expressed by Lord Mount Stephen, in making his munificent donation to King Edward's Hospital Fund, may be speedily realized. Municipal hospitals may, and as a matter of fact do, succeed excellently well in Germany, the paradise of regulations, but in this country the practical application of such a scheme, although most undoubtedly equitable in theory, would be overwhelmed with difficulties. The actual purpose of the hospitals would be ground out of very existence between the upper millstone of over-zealous economy and the nether one of bumbledom (now yclept Moderate or Municipal Reform). The realization of the King's original idea will not only save the hospitals from so dire a fate, but in the Board administering the funds is contained the germ which, under suitable conditions, may eventually develop into a central authority controlling, co-ordinating and bringing into proper organization that *bête noir* of hospitals—the out-patient department.

A battle is raging between Sir Victor Horsley and Mr. Coroner Troutbeck over the subject of inquests on cases which have succumbed after operation. The *punctum saliens* in such cases is whether death has been accelerated or not by the operation. In the former case an inquest is required, in the latter it is obviously unnecessary. Mr. Troutbeck, however, claims that an inquest is always necessary, not only to determine whether death was accelerated, but also to ascertain if anyone has been guilty of negligence or want of skill. Mr. Troutbeck is a lawyer and has been a Coroner for some years. He has a very high opinion of the importance of his exalted office, and has very decided views as to the strictly subordinate position in which the medical profession should be kept in relation to himself. In cases of sudden death he persistently ignores the local practitioners, who in many instances could give him pertinent information, and requisitions the services of an alien from Vienna, a *soi-disant* pathologist, who happens to be his son-in-law. In pursuance of his views on the subject, he summoned Sir Victor Horsley to give evidence at an inquest as to the operation he had performed. In acting as he does and threatening to carry the matter still further, Mr. Troutbeck may be all unconsciously bringing about a change which has for long been desirable. Coroners and their intelligent juries are an utter anachronism and ought to be replaced by the excellent system adopted in Massachusetts, and in several other States of the Union. In the meantime Mr. Troutbeck has taken on rather more than he can manage in crossing swords with one so skilled in fence as Sir Victor.

The Oliver-Sharpay lectures at the Royal College of Physicians were delivered this year by Professor Schäfer, now of Edinburgh University, but formerly of University College, London, who selected for his subject the present condition of our knowledge regarding the functions of the supra-renal capsules. The subject is one of exceeding interest in view of the wide-spread use nowadays in practice of supra-renal extract,

and it was dealt with in masterly fashion, as all who know the Professor would expect. After giving a short account of his old master Sharpey, for many years Professor of Physiology at University College—in whose honour the lectures were founded by Dr. George Oliver—and relating the history of the investigations into the supra-renal capsules which have been carried out during the last four-and-twenty years, the lecturer sketched out in broad outline the evolution of the ideas as to the functions of these bodies and endeavoured to find an answer to the question propounded by the Academy of Sciences of Bordeaux in 1716, "What is the use of the supra-renal glands?" The competing essays sent in at that time were submitted for judgment to Montesquieu. As far as the medulla of these organs is concerned much has been determined, but "our knowledge of the action of the cortex of the capsules is very much in the position which it occupied forty years ago—indeed, one may say in the position it occupied in the time of Montesquieu." It was first pointed out in 1894 by Professor Schäfer, working in collaboration with Dr. George Oliver, that the function of the capsules is to provide an internal secretion, and also that while extracts of the supra-renal cortex are apparently physiologically inactive, those made from the medulla produce a vaso-constrictor action on the peripheral vessels. It is the latter extract which has been subjected to so much investigation of late years and Professor Schäfer proposes that the active constituent of this particular extract shall be called "Adrenin," being a name "without commercial attachments" and, moreover, "it is hardly possible to use a term which will signify the precise chemical constitution of the active substance, this constitution being somewhat complicated." The styptic action of adrenin was first noted and made use of by Oliver and Schäfer in their joint researches into its action upon the blood-pressure. And "one of the most striking facts relative to this styptic action is that, although, when given by the mouth, no evident change occurs in the blood-pressure, there is very distinct evidence of vascular constriction for bleeding from internal parts, such as the stomach, intestine, bladder, and uterus, even the bleeding of *post-partum* hæmorrhage may thereby be effectually controlled. The explanation of this which most readily presents itself is that injured vessels are more susceptible to the extract, and react to a slight excess more readily than do normal vessels." After dealing with the stimulant effect upon the sympathetic system—the difficult point to determine being whether the apparatus or material specially excited by adrenin is limited to the region of the original nerve terminals, or is diffused through the protoplasm—and alluding to the connection of the extract with glycosuria, the lecturer passed on to consider the action on the vessels observing that "the most marked and probably the most important effect of supra-renal secretion in the body is to increase the contraction and maintain the tone of the vessels." This effect varies in the different areas, being greatest in the splanchnic area, and less in the limbs, in which, indeed, passive expansion may be caused by rise of blood-pressure due to the contraction of the arterioles in the splanchnic area. On the pulmonary vessels the effect is far less than on the systemic, and Schäfer sees no physiological contra-indication for its use in assisting to arrest hæmoptysis, when the bleeding is derived from rupture of small vessels, if given by the mouth. The same applies to the cerebral vessels. As to the coronary circulation, the subject is one which requires further examination. At present, the evidence obtained is that it has no action, either constricting or dilating. There can be little doubt that the supra-

renals are related in some way to metabolic changes in the tissues, as indicated by the symptoms of Addison's disease. The extreme wasting and the malnutrition expressed in the abnormal pigmentation of the skin and mucous membrane are not readily referred to the medulla and are probably the result of disease of the cortex. There is some ground for inferring that the cortex may yield a "hormone" which influences the growth and nutrition of certain tissues and organs. Schäfer suggested the integumentary tissues and the generative organs, but declined to plunge further into conjecture, leaving it for a successor to point out "as much positive knowledge regarding the cortex as we now possess regarding the medulla, the function of which seemed no more than fifteen years ago as obscure as that of the cortex appears at present."

July 10th.

PARIS LETTER.

(FROM OUR OWN CORRESPONDENT.)

Perforation of the intestine by ulceration of Peyer's patches, constituting as it does the most formidable of the numerous complications of typhoid fever, is at present engaging the attention both of doctors and surgeons. Among recent contributions on the subject, that of Professor Chantemesse, physician of the Typhoid Fever Hospital, recently communicated to the Paris Surgical Society, is without doubt the most interesting. Professor Chantemesse has observed about 2,000 cases of typhoid fever, of which number there were 35 instances of perforation.

According to his clinical observations, the author arrives at the conclusion that none of the signs which have hitherto, in the classical descriptions of the disease, been attributed to intestinal perforation, rightly belong to it; they are really indications of a peritonitis, and can in fact be noted in cases where there has been no perforation. This is also true of the following symptoms: intense localized pain, disappearance of hepatic dullness, nausea and vomiting, lowering of temperature, quickening of the pulse, and a changed facial expression. Sometimes this complication develops in an insidious manner and then the diagnosis is fraught with difficulties. This being the case are we justified, on the suspicion of a possible perforation, to turn the case over to a surgeon so that the abdomen may be opened, the perforation searched for, and occlusion effected?

If no symptom is pathognomonic of the intestinal perforation complicating typhoid fever, there are nevertheless certain signs which, though small in number, may, if sufficiently acute, have considerable and decisive importance. These are severe pain or intense enough to be localized; retraction of the stomach, and a limited renitency on pressure, indicating at first the possibility of a perforation but not establishing a diagnosis. In the course of some hours, and according to the more or less rapid development of the complication and the intensity of the peritonitis, the abdominal hardness increases, the hepatic dullness disappears, the pulse accelerates, the temperature rises, the facial expression becomes more fixed, and vomiting sets in.

As regards the prognosis of intestinal perforation supervening in these conditions, should we consider it as hopeless, as we have been taught by those whose studies are regarded as classical? Is death inevitable? M. Chantemesse is of the opinion that there has been too much exaggeration for, under treatment, the prognosis is not always unfavorable. That

spontaneous recovery is possible is undeniable. Adhesions to the omentum, to an intestinal loop, to the abdominal parietes, may definitely close the orifice of a perforation. Occasionally the perforation is healed by the formation of an artificial anus. The symptom which should act as the best guide for the doctor is the acceleration of the arterial pulsation: death being a certainty when the frequency of the pulse augments regularly despite the apparently stationary state of the other symptoms. In case the doctor is in doubt he ought to bide his time for, as has only too often happened, surgical intervention may aggravate, instead of ameliorate, the disease. Moreover, certain small perforations get well spontaneously without surgical interference.

As to the question of treatment—and Professor Chantemesse's experience makes him an authority—he holds that to evade the possibility of surgical intervention, recourse should be had to two special therapeutic procedures, of which the physiological end is to increase the number of leucocytes and the peritoneal phagocytes. The first procedure, which is Mikulicz's, consists of the subcutaneous injection of a sterilized solution of nucleinate of soda which, in doses of 50 centigrams administered three times in 24 to 36 hours, produces an onset of transient aseptic fever with increase of white corpuscles in the blood and the peritoneum. In those cases where the microbic culture of the peritoneum is decidedly weak, the afflux of leucocytes shows that the nucleinate of soda causes the desired defense on the part of the serum against infection.

This artificial leucocytic reaction should be combined with a second method of treatment, which consists in intermittently keeping the serum at a high temperature. The effect of this overheating dilates the abdominal capillaries, facilitates diapedesis, increases the activity of the ameboidic movements of the leucocytes; in a word, strengthens the natural defenses of the organism. To achieve the heating of the abdomen the means are very simple: an ordinary ring is used to secure the bedclothes, on the inner side of which is placed a hollowed copper receptacle containing hot water. This apparatus is kept in place from an hour to an hour and a half; the application being renewed two or three times in 24 hours. The benefits accruing from this treatment are, disappearance of pain and the re-establishment of peaceful sleep. The modifications are, movement of the bowels, micturition, lessening of the abdominal distention, and decrease of the peritoneal pain. Under the influence of the nucleinate of soda and the heating of the abdomen, the symptoms are ameliorated in the majority of cases, and after eight days of treatment a cure without an operation is highly probable.

In accordance with what we at present know of the subject, surgical intervention ought to be limited to those cases of intestinal perforation which begin at once with manifestations of a severe peritonitis, and where the symptoms are not lessened in intensity after a few hours. If, as has been said, the use of the bistoury is a declaration of an avowal of weakness, the judicious employment of the two therapeutic methods advocated by M. Chantemesse should be instrumental in diminishing the number of cases where, heretofore, surgical intervention was thought the proper procedure in the treatment of certain complications as they arose in the course of typhoid fever.

July 10th.

OBITER DICTA FROM FOREIGN JOURNALS.

CRIME AND CAPITAL PUNISHMENT.

In his latest book "La peine de mort et la criminalité," Professor Lacassagne shows that he is decidedly in favor of the re-establishment of the death penalty in France. His reasons are excellently put and as for their cogency, admirers of the author's former works will not feel any disappointment. In a recent number of *La Province médicale* appeared an exhaustive review of the book from which the following is excerpted:

Statistics show that the recrudescence of murders which at present obtains in France, will, if it continues, be a disquieting factor in French social life. That the increase in the number of crimes can be laid at the door of alcohol, goes without saying, for under its influence are born the abnormal, the degenerate and all those who have antisocial tendencies. Therefore it behooves society not to disarm in the face of those who have no respect for human life. Moreover, statistics do not tell the whole truth. To complete the list of crimes committed against persons it is necessary to add to those mentioned in the statistics, criminals who have not yet been sentenced. For instance in 1905 out of 355 assassinations and 761 murders, only 169 criminals were sentenced for the former and 274 for the latter. This reckoning would leave 186 assassinations and 487 murders still to be tried. Reprieves, conditional liberation and banishment have not repressed crime to any great extent. As Professor Lacassagne says: "Criminals see in these measures only signs of weakness and fear, since they are too hardened to be easily intimidated. What should they fear? Capital punishment is no longer enforced and then the prisons are such comfortable places! As for the penitentiaries, do they require the hard labor which was once quite insupportable?" And yet in the face of all this crime, society grows more and more tolerant from day to day until the time has now arrived when the persecuted criminal is quite the *rara avis*.

The time is at hand when one may demand in all fairness "if these procedures of indulgence and kindness can be indefinitely applied in a society where uncontrollable impulses and unreasonable disorder, both the offspring of alcohol, are in open revolt against the law?" M. Lacassagne answers his own question in a forceful manner by declaring that "la manière forte" (the strongest means) should be employed. He is of this opinion because though crime is on the increase, the repressive methods as they are to-day, are characterized by diminishing severity. To support his contention that too great an indulgence is shown to criminals, he cites statistics covering about a century. In the reigns of Charles X. and Louis Philippe from 1826 to 1848, commutations amounted to 36 per 100; during the Second Republic, 39 per 100; under Napoleon III. from 1852 to 1870, 46 per 100; during the presidency of Thiers and Marshal McMahon 66 per 100; while Grévy, Carnot, and Félix Faure were presidents from 1881 to 1900, 65 per 100; and finally under Loubet from 1900 to 1906 the number reached 91 per 100. As is well known M. Fallières commutes all those condemned to death. After reading these truthful statements, should we criticize M. Lacassagne for

stating that "the intimidation and the example resulting from capital punishment are effective only when it is often and inexorably applied."

The Restoration is a good example of a time when strong repression was followed by a decided decrease in crime. M. Lacassagne firmly believes that the triumphant march of crime can be stopped only by a rigorous application of capital punishment. The fear of no mercy in the way of commutation would surely arrest the arm of the assassin. To show that there may be a grain of truth in all he says, he is most anxious for the government to make the experiment of abolishing commutation for capital punishment, for a period of six years. The statistics gathered during this period would be the best lesson as to the real efficacy of enforcing capital punishment.

THE GRAVE OF AN OLD COLLEAGUE.

The grave of a physician, who lived 39 centuries ago, the *Lancet* says, has just been discovered in Egypt. The inscription upon his tomb informs us that his name was Nefery. An inventory of the contents of the grave disclosed a rich collection of articles. There were found, besides signs of his profession, a box containing writing pens, two ink bottles, one for red ink and one for blue ink, and a small desk painted and polished in such a way that notes made on it could be wiped out. The inscription tells us that Nefery was a follower of Osiris. His sarcophagus is now placed in the museum at Cairo.

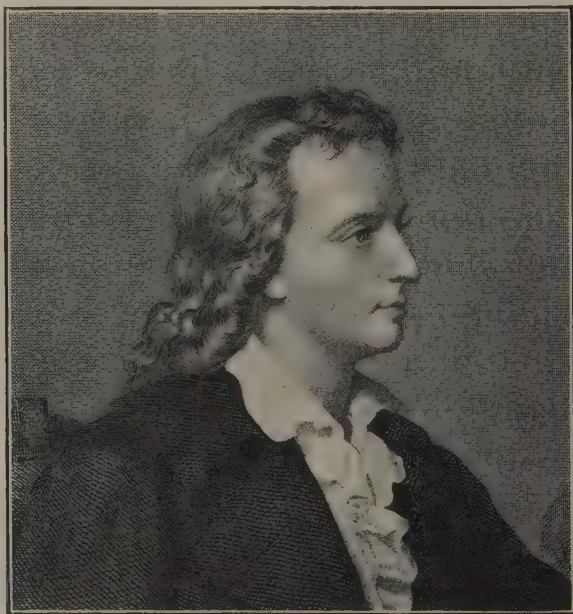
THE NEW CATHETER.

In J. B. van Helmont's "Dageraad oft Nieuwe Opkomst der Geneeskunde," Amsterdam, 1659, there is an interesting chapter, beginning at page 266, descriptive of "Den Nieuwen Catheter" (The New Catheter). To quote: "I saw that the urine was drawn through a bent, silver tube, which was made more rigid by means of a silver thread within. This procedure was very painful, while severe bleeding often took place and on several occasions it was impossible to introduce the instrument into the bladder. The instrument was called 'A Catheter.' I bethought me for some time of using some other material for a catheter but never found any better than the sort that could not be bent without causing pain to the patient and whose introduction demanded considerable skill on the part of the operator. Finally I took narrow linen tape and put this in melted bleached bees-wax. Both sides of the waxed tape were then varnished with linseed oil, which had been boiled with lead oxide. When prepared in this way the wax tape was sewed with a flat seam around a copper thread, making a tube of any length desired. This tube is made more rigid by means of a sound, consisting of whalebone and the outside is varnished once more. Such a catheter can be bent and can be introduced many times a day if necessary. When the catheter is passed through the sphincter of the bladder, the sound is pulled out while a syringe is attached to the part outside. In this way I can empty and fill the bladder as required. A catheter for women should be of larger size but shorter, as the opening to the bladder is practically on the outside. A female catheter is called 'dyoptrinum'—but I do not care for these different names. This is what I want to report for the benefit of all. Somebody else, who is more gifted than I am, will improve my instrument—if God approves of it. 'His name be praised.'"

HISTORICAL NOTES.

SCHILLER AS A PHYSICIAN.

Among the literary men who began their careers as physicians, one would hardly look for that apostle of the Romantic Period in German literature—Friedrich Schiller. But Schiller was the son of an army surgeon, and so thoroughly imbued was his father with the idea that his son should follow in his footsteps, that even decided manifestations of a special aptitude for poetry could not swerve him from his set purpose. Of course it is not difficult to understand that in a case where, as in Schiller's, his mind was on other matters, brilliance would not



FRIEDRICH SCHILLER.

characterize his studies. That this was so is made evident to us in Heinrich Düntzer's "Life," for we read: "At the last examination Schiller had taken no prize, although in anatomy he ranked in equal merit with three others. The lot fell to Elwert; Hoven also gained two prizes. As, however, Schiller's progress satisfied his superiors, he was commissioned to prepare a competition essay for the following autumn, which, if successful, would, so he hoped, secure his dismissal. The subject he chose was one dear to him—The Influence of the Body Upon the Soul. Nevertheless, neither he nor his comrades were suffered to

leave the academy in that year; the duke decreed that they should remain there for another twelve months."

To show more clearly how far his mind was removed from his studies, the great joy he experienced on meeting Goethe for the first time is not without point. In the words of a well-known biographer, "Schiller's heart swelled within him at the sight of Goethe, the favorite of the Muses. Schiller must have drawn deep encouragement and inspiration on seeing the man whose voice had rung so stirringly in the cause of liberty and the pure instincts of humanity, yet who had thus early reached so high and honored a place in life." Is it at all surprising that the five subsequent years of the poet's life—years devoted to the narrow and rather prosaic career of an army surgeon—should have always been recalled as the saddest in his whole career? This is easy of comprehension when we remember that already at the medical school the poet's brain was seething with the subversive social ideas of that strange medley of bombast and exaggerated romanticism—The Robbers. Now what young doctor, starting out on his hazardous career, could retain, under such extraordinary circumstances, the required sanity, without which the practice of medicine is not only uphill work but resolves itself into the dimmallest of callings, with failure written in large letters across its open pages.

That the poet was driven into the practice of medicine by a father whose discipline and military training blinded him to a sympathetic appreciation of unusual gifts, is a lesson in point that has its congeners even in our day. It does not take a deep or very philosophic observer to note that the failures recorded in the world of medical men are oftenest due to the fact that, following advice from parents or intimate friends, men specially unfitted for the calling have embraced it, only to find out, when it is really too late to mend matters, that had they followed the promptings of their own instincts the history of their lives might have been written with a few capitals. Of all natures the poetical has the least adaptability to the prosaic exigencies of the medical man's career, for if there are two callings which are characterized by qualities diametrically opposed to each other, they are surely medicine and poetry. Friedrich Schiller's life contains many things of interest, especially to the student of literature, but the lesson of all lessons for medical men which emanates from it across the many years which have elapsed since his student days, is the tragic one of following a calling, not as he did for a few years, but for a lifetime, as some of us have done and will do, to which we cling from a false sense of duty engrafted upon us by the iron grip of circumstance and the fear of public criticism.

BOOK REVIEWS.

SURGERY: ITS PRINCIPLES AND PRACTICE. In five volumes. By 66 eminent surgeons. Edited by W. W. Keen, M. D., LL.D., Hon. F.R.C.S., Eng. and Edin., Emeritus Professor of the Principles of Surgery and of Clinical Surgery, Jefferson Medical College, Phila. Volume III. Octavo of 1132 pages, with 562 text-illustrations and 10 colored plates. Philadelphia and London: W. B. Saunders Company, 1908. Per volume: Cloth, \$7.00 net; half morocco, \$8.00 net.

This volume is of a high-grade of excellence throughout, the collaborators in almost all instances being the acknowledged authorities on the subjects they treat upon.

Harvey Cushing has the chapter on the "Surgery of the Head," which is exhaustive, covering the subject in a manner that leaves room for little more in the present status of our knowledge. Part I. deals with the External Coverings of the Skull, and Part II. with the Cranium. He considers fractures in a way that will hold the interest of the reader because of a clear descriptive ability, which enhances the thoroughly practical import of the matter. His unilateral or bilateral intermusculotemporal operation in diffuse hemorrhage in basal fractures and his operation for intracranial hemorrhage in the newborn are clearly and well considered. Part III., Meninges and Ependyma, contain three sections of particular note, namely, Hydrocephalus, Sinus thrombosis and Meningitis. Part IV. on the Brain is the most comprehensive and the author reviews here much of the work he and others have done in physiology of compression and localization, and cranio-cerebral topography. After reading the section on Intracranial Hemorrhages and Cerebral Tumors there is a brighter view of this field than ever before. The palliative operations of decompression in unlocalizable growths are often followed by brilliant success, with relief of the general symptoms for long periods and allowing later operation in some cases when the growth becomes localizable. Epilepsy and other sequelæ of injuries and diseases of the brain are clearly discussed. The technic of intracranial operation is elucidated in a well illustrated and excellent descriptive chapter.

The "Surgery of the Neck," by E. Wyllis Andrews, covers the subject well. In the instance of malignant diseases he follows Crile in his insistence upon extensive radical excision with the preliminary clamping of the large vessels, and use of pneumatic suit.

Chapter XXXVIII. on Diseases of the "Thyroid" is by Albert Kocher, and brings out the results of the enormous experience in this work at the clinic in Berne, while the most recent experimental work with the thyroid and parathyroids is commented upon in its clinical bearing. This is one of the most valuable and instructive articles in the volume and carries a well-considered discussion of the indication and types of operations in Graves' disease.

Geo. E. Brewer has embodied the newest methods in his excellent presentation of "Surgery of the Larynx and Trachea and Thorax."

"Surgery of the Breast" is by J. M. T. Finney and is an eminently practical chapter, with the clearest and simplest pathological discussion, and a very definite stand on the proper operative measures, particularly in carcinoma. The position against plastic operations is particularly good. "But any incision designed only to permit a good closure, rather than the removal of a sufficiently wide area of skin is never to be contemplated." The description of the Halsted operation is excellent and the illustrations fair. A typographical error on page 584 makes cancer only 8% instead of 80% of all breast tumors.

In J. Chalmers Da Costa's article on "Surgery of the Tongue," those parts dealing with leukoplakia and cancer are particularly to be commended.

John C. Munro has the chapters on "Technic of Abdominal Surgery" and "Surgery of the Peritoneum and Retro-peritoneal Space." No better person could have been selected to cover this subject, which is now of foremost importance, and in the best form he gives the fruit of his vast experience in diagnosis and operative treatment.

W. J. & C. H. Mayo have the chapter on the "Liver, Gall Bladder and Biliary Ducts." We are familiar with their work about the various parts of the

biliary system and this collection of sections stands out as one of the best chapters in the work.

The "Pancreas and Spleen" are covered in separate chapters by B. G. H. Moynihan, this completing the surgery of the upper abdomen. This field could hardly have been treated better by any living surgeons, and standing as they do the exponents of the most advanced of modern surgery, their word is law, in so far as our science and art permits dictation. The volume is excellently illustrated, and another valuable feature is the complete bibliographies that accompany all of the articles.

Other chapters in the volume are "The Nose and Its Accessory Sinuses," by Harmon Smith; "Mouth, Teeth and Jaws," by Edmund Owen; "Esophagus," by Georg Gottstein.

THE DISEASES OF CHILDREN. A Work for the Practising Physician. Edited by Dr. M. Pfaundler, Professor of Children's Diseases, and Director of the Children's Clinic at the University of Munich; and Dr. A. Schlossmann, Professor of Children's Diseases and Director of the Children's Clinic at the Medical Academy in Dusseldorf. English Translation edited by Henry L. K. Shaw, M. D., Albany, N. Y., Clinical Professor of Diseases of Children, Albany Medical College; Physician-in-Charge, St. Margaret's House for Infants, Albany; and Linnaeus La Petra, M. D., New York, N. Y., Instructor of Diseases of Children, Columbia University; Chief of Department of Diseases of Children, Vanderbilt Clinic. With an Introduction by L. Emmet Holt, M. D., New York, N. Y., Professor of Pediatrics, Columbia University. In four volumes. Illustrated in black and white and in colors by 61 full-page plates and 430 text cuts. Philadelphia and London: J. B. Lippincott Company. Sold by subscription. Price \$20.00 net per set.

This work is the most noteworthy addition to pediatric literature since the appearance of the second edition of Grancher and Cornby's encyclopedic *Traité des Maladies de L'Enfance*. And this work, which is really also a cyclopedic work of reference, is a worthy successor to the other great German cyclopedia of Diseases of Children, the "Handbuch" edited by Gerhardt. Pfaundler and Schlossman's work has been received with so much favor in Germany that the English-speaking profession may congratulate itself that the work has had so excellent a translation.

The work comprises four octavo volumes. Volume I deals with general subjects. After an introductory chapter on general pathogenesis and pathology, by Hamburger of Vienna, there comes a chapter which is unique in pediatric literature. This is a chapter on Symptomatology of Children's Diseases by Pfaundler. This semiotic summary is really a marvel of clearness of exposition, of presentation of detail, and of massing of information. The symptomatology is worked out under 206 separate headings, with elaborate indices and cross-references. In each division the symptom is described, directions for distinguishing it given, data concerning respective physiological conditions at different ages added. Predispositions whereby the symptom might be simulated, are also discussed.

While the author expressly (and wisely) states that the work is not supposed to afford an automatic index or "key" to the diagnosis in a given case, there can be no question of the value of this particular work with reference to the discussion of the various relations and significations of different symptoms. In many cases, elaborate tables have been prepared to facilitate the study of special symptoms, e. g., vomiting, albuminuria, etc. Other chapters in the general part concern themselves with discussions of general prophylaxis, therapeutics and mortality in infancy and childhood.

The rest of the first volume is taken up with the presentation of the problems of nutrition and metabolism in infancy and childhood. The articles by Camerer on metabolism and nutrition and on growth and weight during the first year, are particularly noteworthy for their careful and authoritative presentation of physiologic problems.

Volume II opens with an excellent article on the diseases of the newborn, by Knopfmacher. Of the general articles in this volume, the chapter on puberty and its physiology and pathology, by Professor Seitz of Munich, is especially noteworthy. The acute exanthemata are all considered in detail, the various illustrative plates being exceptionally good and very numerous. Schlossman's article on tuberculosis in infancy and childhood, is a most excellent presentation of present day authoritative opinion on this most important subject.

In Volume III, diseases of the gastrointestinal tract are discussed in detail.

Chapters of especial interest are those on the diseases of nutrition in infancy, and on gastrointestinal disorders in older children, by Fischl, of Prague, and on pyloric stenosis in infancy, by Pfaundler himself. The second half of this volume is taken up by the consideration of diseases of the respiratory and circulatory systems.

Volume IV is taken up (1) with the presentation of diseases of the uropoietic system; (2) with an exhaustive study of the diseases of the nervous system in childhood; and (3) with a description of the more important skin lesions in childhood.

A complete and carefully prepared index for the work is also included.

Detailed discussion of all these chapters is out of the question, but it is safe to say that the work of Pfaundler and Schlossman will at once take the place with the English-speaking profession which the original did in Germany, that of an authoritative work of reference on present day pediatric knowledge.

As a whole, this work is one that can be used with the greatest satisfaction as a work of reference by all physicians having to care for children. It is a pleasure to be able to refer to the almost uniformly good English of the translation. The book work is fully up to the standards of the publishers.

A HANDBOOK OF SUGGESTIVE THERAPEUTICS—APPLIED HYPNOTISM—PSYCHIC SCIENCE. By Henry S. Munro, M. D. C. V. Mosby Co., St. Louis, 1907.

A glance at the title of this book invites its own criticism. To the discerning reader and to the informed physician that would be all that is necessary to warn him from purchasing and certainly from reading this book. This book is the product of the professional hypnotist, a second cousin to the variety hall performer and the country fair experimenter. It is full of bad English, ill considered egotism and distorted facts. The only praiseworthy feature of the book is that it does not include within its pages a full length portrait of the professor in evening clothes in his favorite attitude of hypnotising a self-conscious, respectable but otherwise harmless physician. This would be about the only truthful thing in the book. For the sake of the momentary joy this picture might awaken in some of the discerning, it is to be regretted that the author has seen fit to omit it. The book reflects credit neither upon the publisher nor upon that portion of the medical profession which it is designed to reach.

A MANUAL OF ORTHOPEDIC SURGERY. By Augustus Thorndike, M. D., with 191 illustrations. P. Blakiston's Son & Co., Philadelphia.

This is a small book and is a very creditable attempt to present orthopedic surgery in a simple way to the student and practitioner. The errors of development in the skeleton and nervous system, the deformities due to weight-bearing, improper restraint from clothing, asymmetrical muscular development, as well as those dependent upon acquired diseases, are treated from an etiologic and chronologic point of view. There is a practical description of the technique of plaster-of-Paris bandages and the fitting of orthopedic appliances. The book is small enough to be carried in the pocket and is an excellent manual.

STATE BOARD QUESTIONS AND ANSWERS. By R. Max Goepf, M. D., Professor of Clinical Medicine at the Philadelphia Polyclinic. Octavo Volume of 684 pages. Philadelphia and London. W. B. Saunders Company. 1908. Cloth, \$4.00 net; Half Morocco, \$5.50 net.

This work is created for the purpose of aiding those who anticipate appearing before the various boards of examiners, such as state licensing boards, internship in hospitals, and the Army and Navy medical service. Dr. Goepf has collected a great quantity of questions propounded in the different examination rooms and appended appropriate answers. The book fulfills its mission admirably and will be of great assistance to those who desire to apply for appointment to positions through competitive examination.